THURSDAY, OCTOBER 4, 1877

MICROSCOPICAL PETROGRAPHY

Microscopical Petrography. By Ferdinand Zirkel. Being Vol. VI. of the Report of the United States Geological Exploration of the Fortieth Parallel made under the direction of the Engineer Department by Clarence King, Geologist-in-charge. (Washington, 1876.)

TO the massive and important series of volumes in which the Report of the Exploration of the Fortieth Parallel has been published the Engineer Department of the United States has just added a sixth which, for general interest and usefulness beyond the area of the Survey, is equal if not superior to any that has preceded it. In the course of this protracted and laborious survey many rocks were encountered to which Mr. Clarence King and his coadjutors felt somewhat at a loss to apply the petrographical nomenclature of Europe. He accordingly sought help from the highest European authority on the subject, Prof. Zirkel, of Leipzig, whom he induced to undertake the task of examining the vast collection of rock-specimens which had gathered during so many years of field-work. Prof. Zirkel accordingly crossed the Atlantic, spent some time in New York with Mr. King and his staff in making a preliminary investigation of the collection, and in learning the geological position of the specimens and the geological structure of the wide region from which they had been obtained. Subsequently a large and typical series of rock-specimens was sent over to Leipzig to be submitted to careful microscopical investigation. No fewer than twenty-five hundred thin sections were prepared and examined under the microscope. The result of Prof. Zirkel's laborious task is now given to the world and most appropriately forms a separate volume of the Report on the Geology of the Fortieth Parallel. Mr. King may be congratulated upon the judgment he has shown in the allocation of his materials. He has enriched his official publications with the most important contribution yet made to the petrography of America.

Of the way in which Prof. Zirkel has acquitted himself of the task he undertook, it is hardly possible to speak too highly. With the characteristic method of his countrymen he marshals his facts in such orderly fashion that every observation has its appropriate and proper place where it may be expected and where, if sought for, it will be found. Familiar as he is with the minute texture and composition of most European rocks, it must have been a congenial, even though laborious work, to attack on such a scale those of another continent. He has evidently given himself heartily to the investigation, and has produced a work which more than sustains his well-earned reputation.

In an introductory chapter the author briefly sketches the leading types of microscopic structure which, largely as a result of his own previous labours, have been recognised among crystalline rocks. These may be reduced to three :—1. The purely crystalline, that is, rocks which display only crystals or crystalline particles so interwoven as to form a solid, compact mass. Granite may be taken as the type of this group. 2. The half-crystalline. Rocks of this group consist partly of crystals or crystalline particles, and partly of a non-crystalline amorphous substance or paste, which may be (*a*) a colourless but more usually yellow, brown, or grey glass ; (*b*) partly devitrified by the appearance of minute translucent but non-polarizable grains (globulites), or variously-shaped opaque needles or hairs (trichites) ; (*c*) still further devitrified by the increase of these grains and needles, so that little or no glass remains—a structure termed micro-crystallitic ; or (*d*) a peculiar amorphous substance neither showing the transparency of glass nor definite grains and needles (crystallites), but appearing to consist of indistinct grains or fibres, which seem to melt into each other. This is termed the microfelsitic. 3. The non-crystalline. Here the rocks consist sometimes merely of glass, as obsidian, sometimes of the amorphous microfelsitic substance, as in felsites. Dr. Zirkel admits, however, that even where these differences of minute structure are best shown they do not suffice as a basis for the systematic arrangement of rocks, which must rest on fundamental mineral constitution. The same mass of rock, indeed, may within a short space put on extraordinary diversities of minute structure.

A number of terms are introduced into the Report which, though most of them have for some time been in use in Germany, for the most part make their first appearance here in an English dress. "Ground mass" is employed to denote what seems to the naked eye to be the dense homogeneous matrix of a rock, wherein the usual scattered porphyritic crystals are held ; "base" is used as the designation of what is only seen under the microscope to be a non-crystallised or unindividualised paste, glassy, globulitic, micro-crystallitic or micro-felsitic, as the case may be, in which the crystals, whether microscopic or visible to the naked eye, are held. "Macroscopic" has obtained wide currency in German petrographical literature as a convenient designation for what can be seen without the use of lenses. "Microlites" are minute, thin, needle-shaped, usually cylindrical bodies, which occur both in the base and in separate crystals of rocks, and represent imperfect stages in the crystallisation of different minerals ; when colourless they are called "belonites," when black and opaque, "trichites."

As most rocks have undergone more or less internal alteration, many products of decomposition are met with under the microscope which cannot always be identified with definite mineral species. No one who has practically studied microscopic petrography can fail to have been often puzzled to name some of these products. They are in far too minute quantity and too intimately diffused through the substance of a rock to be capable of being collected for chemical analysis. They present no recognisable crystallographic form, and they show no distinctive reaction under the polariscope ; yet they have too often, with no expression of hesitation, been identified with known minerals, the identifications being at the best only guesses, and sometimes most improbable ones. It has lately been the practice at Leipzig to avoid attempting such identifications when the evidence is so slight, but to be content with the application of provisional names which may include many different compounds having at least some common characters, such as opacity or colour, and to wait until the progress of investigation allows more precise names

to be affixed. Prof. Zirkel now introduces these provisional and useful terms to English readers. "Opacite" includes all the black opaque amorphous grains, scales, and streaks which have resulted from the decomposition of different minerals, and which, no doubt, vary widely in chemical constitution. They probably in most cases consist largely of metallic oxides. "Ferrite" embraces those yellowish, brownish, or reddish specks, grains, veinings, or pseudomorphous crystals which occur in so many rocks where oxides of iron have decomposed. "Viridite" is the term applied to greenish transparent or translucent scales, fibres, or veins, frequently seen where hornblende, augite, or olivine have been altered. They must vary much in composition, sometimes approaching chlorite, sometimes delessite or serpentine.

These scientific terms may be usefully transplanted into English text-books. The only one which, though the great need of such a word cannot be denied, seems open to considerable objection, is "macroscopic." It is too like "microscopic," whether as written, printed, or spoken. "Gymnoscopic" would be better. But there occur throughout the Report many nouns and adjectives which the reader will in vain look for in any dictionary, and the meaning of some of which he will not readily appreciate if he does not happen to be familiar with the German petrographical terms for which they are intended. Such are "fibration," "lamellation," "inclusion," "zonally," "lineated," "fluidal," "interwedged," and many more. Even ordinary words are used in a way which is apt to puzzle the uninitiated. For example, "some occurrences are poor in augite," "poorly-shaped crystals," "drop-like or crippled minerals." The English language is not quite so meagre as to be unable to furnish expression in already familiar words and phrases to the ideas sought to be conveyed by these novel and sometimes rather uncouth terms.

After a brief chapter devoted to the crystalline schists and their related rocks, the author proceeds to what are commonly known as the igneous rocks, beginning with granite and the early intrusive porphyries and felsites, passing thence through the diorite, diabase, gabbro, and other groups, into the wide series of tertiary volcanic products. V. Richthofen's name propylite is retained for the oldest eruptive rock of the tertiary series—a mixture of plagioclase felspar with hornblende, having most of the characters of the old diorites and dioritic porphyries. The petrographical differences between this rock and andesite are carefully summed up by Prof. Zirkel; but at the most they appear to be rather fine-drawn. He insists that rocks of different geological date can be distinguished petrographically, and that this may be done even among the different members of the tertiary series. Undoubtedly the most important chapter of the Report is that devoted to the trachytic and rhyolitic rocks. Among the trachytes some have been found containing augite instead of hornblende—a curious and novel fact which establishes an analogy between these tertiary masses and some old syenites of Tyrol and Norway, in which G. von Rath has lately shown that augite replaces hornblende. The author partly following von Richthofen divides the rhyolites into (1) Nevadite or granitic rhyolite; (2) Rhyolite proper, including the felsitic and porphyritic varieties, of which he has found among the rocks of the Fortieth Parallel no fewer than sixteen well-defined types; and (3) Hyaline rhyolite, including the glassy and half-glassy varieties, obsidian, pitchstone, pumice, &c. With the exception of some varieties in the eastern part of the region, all the basalts met with in the course of this survey prove to be felspar-basalts. Though repeating in Western America the familiar characters of the basalts of Western Europe they contain some varieties which merit a special subdivision. These are marked by (1) the invariable presence, though in small quantity, of sanidine, (2) the general absence of olivine, (3) the abundance of the glassy microlitic base, (4) the occasional presence of hornblende, (5) a high proportion of silica, (6) the dusty character of the included apatite. A petrographer who admits such wide departures from the normal type of a species must not be surprised at those who would further seek to unite some of his species which hardly differ from each other so much as these varieties of basalt do.

The Report is illustrated by twelve quarto coloured plates. For beauty of execution nothing has appeared like them since those of the lamented Vogelsang. They have been executed at Leipzig, under the author's own eye, and are evidently as faithful as they are vivid and artistic.

ARCHIBALD GEIKIE

OUR BOOK SHELF

Results of the Aralo-Caspian Expedition. Fascicule iv., 388 pp., with seven lithographed plates; and Fascicule v., sixty-eight pages. (St. Petersburg, 1877.) [Russian.]

THE fourth fascicule of this publication contains an important paper by the well-known Russian ichthyologist, Prof. Kessler, on "The Fishes of the Aralo-Caspian Pontic Ichthyological Region." After an introduction, in which the author briefly sketches the geography of the region, and makes a few objections to some statements of Mr. A. R. Wallace as to the geographical distribution of fishes, Prof. Kessler describes forty-three new species and varieties of fishes of the region, and twenty-four other species, the previous descriptions of which were incomplete. These descriptions, being the result of very elaborate researches, are based on extensive collections obtained by the members of the expedition, and by previous explorers. The new species are illustrated by seven plates. The second part of the work is a systematic catalogue of all fishes known to inhabit the region, with notes as to their geographical distribution.

The third part deals with the general conclusions arrived at by the author as to the geographical distribution of species, the relations of the Aralo-Caspian ichthyological fauna to the faunas of the neighbouring basins, the distribution of species in different waters of the region, the zoological characteristics of the fishes inhabiting it, and their genealogical relations, their mode of life, and some remarks on the geological history of the region. These conclusions (some of which have already been noticed in NATURE) will certainly be of great interest to the zoo-geographer, and their importance is much enhanced by the usual caution of M. Kessler's statements. The work is altogether an important acquisition to ichthyological literature in general, all the more that it deals with countries very imperfectly known until now.

The fifth fascicule of the work contains two papers by M. Alénitzin: "On the Sweet Water Springs on the Shores of Lake Aral," and a "Sketch of the History of the Islands of that Lake," the former containing some interesting information as to the distribution of water in sandy steppes.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Urticating Organs of Planarian Worms

THERE exist, as is well known to all comparative anatomists, in the skin of most planarian worms certain rod-like bodies (Stäbchenkörperchen of German authors) concerning the function and homologies of which there has been considerable speculation and difference of opinion. By some authors these bodies, which always at an early stage of their existence are contained in cells, "the rod-cells" have been compared to the thread-cells or nematocysts of cœlenterata, the rod-cells being considered homologous to or possibly homogenous with these cœlenterate nematocysts. In the July number of the *Quarterly Journal of Microscopical Science*, vol. lxvii., new series, 1877, I published a paper on the structure of several forms of land planarians obtained by me during the voyage of H.M.S. *Challenger*. In this paper is described and figured the structure of the rod-cells of several genera of land planarians as observed in the fresh and living condition. In an American form *Geoplana flava* and also in a *Geoplana* from New Zealand and a *Rhynchodemus* from the Cape of Good Hope rod-cells were observed in which the rods are much longer than the cells and are in their quiescent condition coiled spirally within the cells (*l.c.*, Pl. xx. Figs. 15, 20, 21, 22, 23), but which rods are shot out from the cells and protruded for a long distance beyond the surface of the epidermis when the animal is compressed or irritated. Such probably is the mature condition of the cells in question in all land planarians. Mecznikow has described a somewhat similar form of cell as existing in his *Geodesmus bilineatus*.

In some microscopic sections of land planarians hardened in alcohol, the shot-rods or threads may be seen in abundance when closely looked for, projecting from the edges of the section of the epidermis. The demonstration of the spiral coiling of the rods within the cells, and of their protrusion on irritation, would at first sight seem to ally these bodies more closely than ever with cœlenterate nematocysts, but there is this great difference between the two structures, that several rods are present in each cell in the planarians, and that the rods are solid and apparently free within the cell, and when protruded by the bursting of the cell are shot clear of it. In cœlenterates, as is familiarly known, the thread is continuous with the cell and hollow, and is everted in the act of protrusion.

In the summary of my paper above referred to (*l.c.*, p. 292) I suggested that it would be interesting to test the action of the rod bodies of land planarians by applying a living worm to the tongue and observing whether urtication was produced. I wrote at the time to my friend, Mr. Thwaites, F.R.S., curator of the Royal Botanic Gardens at Peradeniya, Ceylon, and asked him to make the experiment, which he did forthwith, and the result shows that planarians do undoubtedly produce urtication in much the same way as cœlenterates, and there can be no doubt that this function is performed by the rod-bodies, which are thus weapons of defence, and no doubt used also to secure prey.

Mr. Thwaites writes :—"I have lost no time before attending to your request touching the planarians. I applied the tip of my tongue to two of them brought fresh and lively to me, and quite sensible was I to a feeling of unpleasant tingling, and it was accompanied with slight swelling. The sensations very similar to what is experienced upon a slight scalding. The planarian itself evidently felt very uncomfortable, as it became dilated laterally to a considerable extent during contact with the tip of the tongue, though it soon recovered its normal condition."

H. N. MOSELEY

New University Club, St. James's Street, S.W.

The Satellites of Mars

IT seems worthy of notice that the prophetic genius of Homer has already not only identified but even given names to the two satellites of Mars. I allude, of course, to the passage in the fifteenth book of the Iliad, where Ares is preparing to descend to the earth (possibly this refers to an unusually near approach at opposition, as at the present time) :—

Ὣς φάτο· καί ῥ' ἵππους κέλετο Δεῖμόν τε Φόβον τε
ζευγνύμεν. . . *Il.*, xv. 119.

which Pope renders—

"With that he gives command to Fear and Flight
To join his rapid coursers for the fight."

Deimus and Phobus are not, perhaps, very euphonious names; but astronomers will not lightly reject the authority of Homer.

Eton, September 29 H. G. MADAN

On the Coming Winter

HAVING recently computed the remaining observations of our earth-thermometers here, and prepared a new projection of all the observations from their beginning in 1837 to their calamitous close last year—results generally confirmatory of those arrived at in 1870 have been obtained, but with more pointed and immediate bearing on the weather now before us.

The chief features undoubtedly deducible for the past thirty-nine years, after eliminating the more seasonal effects of ordinary summer and winter, are :—

1. Between 1837 and 1876 three great heat-waves, from without, struck this part of the earth; viz., the first in 1846·5, the second in 1858·0, and the third in 1868·7. And unless some very complete alteration in the weather is to take place, the next such visitation may be looked for in 1879·5, within limits of half a year each way.

2. The next feature in magnitude and certainty is, that the periods of minimum temperature, or cold, are not either in, or anywhere near, the middle time between the crests of those three chronologically identified heat-waves, but are comparatively close up to them *on either side*, at a distance of about a year and a half, so that the next such cold wave is due at the end of the present year.

This is, perhaps, not an agreeable prospect, especially if political agitators are at this time moving amongst the colliers, striving to persuade them to decrease the out-put of coal at every pit's-mouth. Being, therefore, quite willing, for the general good, to suppose myself mistaken, I beg to send you a first impression of plate 17 of the forthcoming volume of observations of this Royal Observatory, and shall be very happy if you can bring out from the measures recorded there, any more comfortable view for the public at large.

PIAZZI SMYTH
Astronomer-Royal for Scotland

Royal Observatory, Edinburgh, September 27

The Australian Monotremes

I OBSERVE in NATURE (vol. xvi. p. 439) that a doubt arises respecting the *Echidna* or Australian porcupine (recently re-named *Tachyglossus*) and the *Ornithorhynchus* being found in Northern Australia. It does exist in Queensland, but how far *north* it is impossible to decide until we are better acquainted with that extensive territory. The fact of one having been found by Mr. Kennedy, as mentioned by Mr. Forbes at Plain Creek in lat. 21° S. is, as far as the published statement can be depended upon, correct, and was never considered by any Australian in Queensland as a matter of doubt, as they were well acquainted with the animal; but whether the *Tachyglossus* was the same or of a different species I do not consider has been sufficiently noticed; whether it was the *Tachyglossus hystrix*, or with sufficient distinctive characters, as has been recently found in that of New Guinea to make it a new species, is not known, as ordinary travellers are not able to distinguish those characteristic differences which would immediately strike the experienced naturalist. The species found in the vicinity of Darling Downs, &c., is evidently the *Tachyglossus hystrix*, and from a recent letter received from my son, Mr. G. F. Bennett, he finds no difficulty in procuring specimens of this species near Foowoomba by offering rewards for those procured at certain intervals of time, to enable him to carry out his investigations on the mode of generation of the *Monotremata*, and if possible to procure the impregnated uterus of that animal, as well as that of the *Ornithorhynchus*, as in both animals it no doubt will be identical. As far as regards the rudimental pouch in the *Echidna* it is only able to be found in that animal during the breeding season, and I could never detect it at any other time. It is mentioned by Prof. Owen in his memoir on the young of the *Echidna* (*Philosophical Transactions*, 1865, p. 678), and indeed it has been a well-known fact for some period of time, as some years ago I doubted the assertion and public attention was most particularly drawn to it, and the fact was ascertained beyond doubt even before the publication of Prof. Owen's paper.

The *Ornithorhynchus* being an aquatic animal does not possess a pouch at any time. With respect to the New Guinea species of *Echidna*, the question whether the *Tachyglossus lawsei* and *T. bruijnii* are distinct species can now be decided, as I observe that examples of both sexes of *T. bruijnii* have been obtained in the mountains on the north coast of New Guinea at an elevation of about 3,500 feet. That a new and somewhat analogous species of *Tachyglossus* may yet be discovered in Northern Australia I consider very probable. GEORGE BENNETT

September 29

P.S.—By letters from Sydney dated August 4 no intelligence has been received from Sig. D'Albertis since his departure for the Fly River in May last.

Are there no Boulders in Orkney and Shetland?

IN your impression of the 13th inst. (p. 418), there is an interesting letter from Mr. Laing, M.P., stating that there are no boulders in Orkney or Shetland. He says that having "an intimate personal acquaintance with these islands, which are my native county, and almost every yard of whose surface and shores I have explored with rod and gun," . . . "I can assert positively that I never saw (in them) a boulder or perched block, or the trace of any till, or boulder clay, kame, eskar, raised beach, or other form of glacial or marine action."

Mr. Laing's object in drawing the attention of geologists to these facts is, that "if true, they seem to afford a crucial test of the truth or falsehood of some of the most important theories of modern geology."

Mr. Laing observes that in Orkney there could be no boulders, &c., because "there were no glaciers, there being no great local mass of mountains to produce them."

As regards Shetland, Mr. Laing says he cannot speak with the same confidence. "Still, having travelled over a great part of the principal islands, I can assert that I have never seen in them either, any traces of glacial action."

Mr. Laing having invited information on this subject, Prof. Geikie has published an article in the same number of your paper (p. 414), controverting Mr. Laing's statements, and maintaining that the facts ascertained by him and his colleagues in the Scotch Geological Survey establish that these islands form "no exception to the general glaciated condition of Scotland."

In corroboration, so far, of the Professor's statement, that there are in Orkney and Shetland "many transported blocks of gneiss, schist, and other rocks foreign to the immediate locality" of the blocks, I need only refer to the following list of boulders reported to the Edinburgh Royal Society Boulder Committee.

IN ORKNEY.

Eday Island.—Conglomerate B. 12 × 7 × 1½ feet, = about 8 tons weight. Situated near top of a hill 250 feet above sea. No conglomerate rock in Eday, but there is in Stronsay Island.

Frith and Stennis.—White pebbles of freestone on the hills. But there are no white freestone rocks in this island. It is all old red.

Sanday.—Gneiss B. 7 × 6 × 2½ feet = about 14 tons. No gneiss rocks on this island. Nearest island with gneiss rocks is Stromness, 30 miles distant, and in Shetland, still more distant.

Walls.—Lydian stone B. 9 × 7 × 6 feet = about twenty-eight tons. Sandstone is the prevailing rock.

Stromness Island.—Two granite boulders lying on red sandstone rocks—distant, one a quarter of a mile, the other one mile from granite hills.

IN SHETLAND.

Bressay.—A number of boulders here, of a rock foreign to the island. One of them is 10 × 7 × 4 feet. Supposed to have been transported from Norway.

Housay.—On a cliff 200 feet above the sea, rounded blocks resting on knolls of polished rock.

Neay.—Large perched blocks, some many tons in weight.

Sumburgh Head.—Conglomerate B. lying on sandstone rock.

Where can it be supposed that these boulders come from?

Prof. Geikie thinks there were glaciers, at all events, in one of the islands, viz., *Hoy*, and even "separate glaciers" in all the valleys of that hill, whose top is only 1,550 feet above the sea. I feel great difficulty in subscribing to that opinion; I rather agree with Mr. Laing, that there could be no glaciers, for want of a sufficient "mass of mountain region to produce them." Even if in Hoy glaciers could have been formed on a hill the highest peak of which is only 1,550 feet above the adjoining sea, what is to be said of those boulders which are on islands where the hills do not exceed 500 or 200 feet, and in which there are no rocks of the same nature as the boulders?

Prof. Geikie refers to the rock striations in Orkney and Shetland (which Mr. Laing seems not to have discovered) as additional proofs of glacial action. If these striations had been caused by glaciers, the direction of the striæ would vary with the direction of the different valleys in which the glaciers moved. But this is found not to be the case. Prof. Geikie says that both in Orkney and in Shetland the movement of the ice has been on the "whole along a north-west and south-east line." He refers to reports by his colleagues, Mr. Peach and Mr. Horne, in corroboration of his statements.

In looking into Mr. Peach's report, I find that he specifies the striæ on the rocks of Shetland as running in *various* directions. In *Unst*, the most northern island, he says "the destroying force (the nature of which force, however, he avoids indicating) coming against a hill (called the '*Muckle Heog,*' 500 feet high) on its north-west flank, had been partially *turned* by the hill into a valley (which he names) and made to produce the well-known phenomenon of 'crag and tail'"—the crag or bared rock being on the north side of the hill.

Mr. Horne in his paper also describes the striæ in Shetland as running in *various different* directions. Some of the striæ on the rocks, and the boulders on the surface, indicated, as he thought, ice action from east to west. "In addition to these, however, *others* (he says) were found, which could not have been produced by ice-shedding in the ordinary way. These cross the island, *regardless of its physical features,* and are *often at right angles* to the newer set."

These facts, I venture to submit, may be explained by supposing that the Shetland and Orkney Islands, when still under the ocean, were subjected to the action of Arctic currents loaded with icebergs and shore ice. We know that in the Arctic regions now, fragments of rocks are by these means carried about in various directions, and dropped on the sea-bottom; whilst the rocks at the sea bottom are ground down, polished and striated by the hard stones and gravel pushed forward by icebergs. The existence of Arctic currents from north-western points has indeed been already well established by a study of the boulders and striated rocks found along the west coasts of Caithness, Ross, Argyll, and the islands of Lewis, Harris, and Uist.

The inference of Mr. Peach from what he saw near Lerwick was, that there "the *direction* the *drift* came from is evidently *northerly.*" "The destroying force" to which Mr. Peach refers as having swept across the island of Unst baring a hill up to a height of 500 feet *on its north-west* flank, could have been no other than an Arctic current loaded with ice.

These facts establish points of the highest geological interest. They indicate a submersion of the northern parts of Europe under the ocean to the extent of many hundred feet, and the non-existence of any gulf-stream flowing through the North Atlantic. The Isthmus of Panama requires to be depressed only 350 feet, to allow that stream to flow into the Pacific.

The separate question of "*Raised beaches*" mooted by Mr. Laing and discussed by Prof. Geikie, I do not enter on. Both of these authorities agree that there are no raised beaches in Shetland and Orkney. It is indeed very curious that such should be the case, considering that they exist along the Caithness coast, and in every other part of the kingdom, including Ireland. I may, however, notice that Mr. Peach in hisReport on Shetland speaks of a "raised beach" as having been seen by him there.

Milne Graden, Coldstream, N.B. DAVID MILNE HOME

Fertilisation of Flowers by Birds

AMONG the "Biological Notes" in NATURE, vol. xv. p. 416, there is one referring to the agency of birds in effecting the fertilisation of flowers. A few weeks before reading this note I was induced to suspect that many flowers might be dependent wholly or in part on the visits of small birds for their effectual fertilisation by observing that a very considerable number of birds shot at that time had the plumes of the forehead and the lores thickly dusted with pollen. This fact was noticed in several species of *Dicæinæ* and *Nectariniinæ*, in the *Loriculi*, and even in a glossy starling (*Calornis panayensis*), which latter is mainly a frugivorous bird. Both the sun-birds and flower-peckers are fruit-feeders to a certain extent; but they also prey on minute insects, in search of which (and possibly of the nectar sometimes) they diligently probe the corollas of numerous flowers, and on withdrawing their heads a portion of the pollen remains in many instances adhering to the plumage bordering the bill, and is carried away and introduced into the next blossom visited.

The immediate source of attraction possessed by the flower for its feathered visitants lies, I think, in the small insects which resort to it, and not, at any rate usually, in their furnishing any nectareous secretion which is palatable to the birds. For if the latter visited the blossoms for the sake of the nectar they would be perfectly acquainted by experience with its situation and make no delay in going straight to it, whereas the habit of the sun-birds and the flower-peckers also is rather to hover on rapidly-vibrating wings a few inches in front of the opening of a blossom, as if prying into its recesses in search of food, before thrusting their beaks into the corolla; and often after thus examining a flower they fly off to another without touching it at all, having apparently satisfied themselves that the first one contained no prey for them.

A. H. Everett

N. Mindanao, July 23

Heat Phenomena and Muscular Action

On reading the article which appeared in Nature, vol. xvi. p. 451, on the heat phenomena accompanying muscular action, it has occurred to me to send the following problem which is akin to the subject.

If a man does work (say lifts a weight), the principle of the conservation of energy teaches us that the potential energy—the work done—(weight lifted) is at the expense of the man as a magazine of force, in fact that "virtue has gone out of him." Now suppose a man lifts say a ton of bricks and deposits the bricks one by one on the top of a wall six feet high, we can exactly estimate the amount of work done, the energy rendered potential and external, and if we knew also the extra amount of heat radiated or otherwise carried off from his body—as most probably the work would raise his temperature—we could exactly measure the amount of energy the lifting of the brick cost him.

Now suppose another man were to lift the bricks from the top of the wall and deposit them gently—*i.e.*, without concussion—on the ground, it is evident that there is a certain amount of potential energy disappearing, in fact that there is work being absorbed by the man, of course appearing in some other form, but the question is how? This second man's work is of course in one sense work, but in the sense of producing external, potential, or kinetic energy, is not so, unless, perhaps, in heat.

Strangely enough it follows that lifting down the brick ought to make the man either radiate heat more, waste tissue less, digest food less, or in some other way account for the energy absorbed by him.

Generally I think the conversion of force by obstruction is not always so clearly traced as it might be; in friction it is clear, as also in the compression of elastic bodies, but in the instance above, as also in the throttling of steam, it is not so clear.

A. R. Molison

Does Sunshine Extinguish Fire?

I read Mr. Tomlinson's paper (Nature, vol. xvi. p. 361) near the time of its delivery, and was struck with the inconclusive character of his experiments. What he attempted to obtain was the condition of combustion in sunshine and combustion in darkness, *cæteris paribus*. But he left the *cæteris paribus* entirely out of the experiment, and actually used a dark cubbard (I believe this is good spelling etymologically and phonetically), into which there was no free influx of atmospheric air. Naturally his candles burnt with inferior combustion there. I have for years together burnt Newcastle coal, and no other; and for years together burnt South Staffordshire coal, and no other; and I say that sunshine puts out a sea-coal fire and not a S.S. fire. The reason of this is, I apprehend, not far to seek. In the Midlands it is the practice to keep a fire alive by a raker, or gathercoal. It would be quite useless to attempt to do this with a sea-coal fire, which goes out in a short time unless the cakes of coal be broken up; in a word, one has to watch a sea-coal fire; and it must be in every Londoner's experience, that such a fire is apt to elude one at the last faint gleam from over reckless poking. Now, if the sun is shining on the coal, that last faint gleam is invisible, and the fire goes out as a matter of course. Sunshine puts out a sea-coal fire by insidiously eclipsing the warning glimmer of its expiring embers. This, at least, is a *vera causa*. *A priori* I should say that combustion would be less rapid in air rarefied by sunlight than in air deprived of it; but I do not believe sunshine extinguishes a coal fire in any other way than that I have described.

C. M. Ingleby

Folkestone

OUR ASTRONOMICAL COLUMN

The Approaching Opposition of Iris.—The opposition of this minor planet in the present autumn affords another favourable opportunity of determining the amount of solar parallax on the method already successfully applied by Prof. Galle, of Breslau, in the case of Flora. The *Berliner astronomisches Jahrbuch* for 1879 contains a rough ephemeris of Iris for every twentieth day of the year, but this being insufficient for the purpose in view, we subjoin places calculated from Prof. Brünnow's tables of the planet, on the approximate formulæ explained in his introduction; the error of the tables being very sensible at the present time, nothing would have been gained by calculating in the accurate form. For the sake of brevity the planet's positions are given for every fourth day only, but they will be readily interpolated for the intermediate dates.

Iris.—*At Greenwich Midnight.*

	Right Ascension. h. m. s.	North Declination. ° ′	Distance from the Earth.	Distance from the Sun.
Oct. 8	3 56 7	27 5·6	1·0034	1·8350
,, 12	3 56 50	27 3·3	0·9769	
,, 16	3 56 55	26 57·8	0·9526	1·8345
,, 20	3 56 20	26 48·9	0·9306	
,, 24	3 55 8	26 36·5	0·9111	1·8354
,, 28	3 53 20	26 20·5	0·8944	
Nov. 1	3 50 58	26 0·9	0·8808	1·8376
,, 5	3 48 8	25 37·8	0·8704	
,, 9	3 44 57	25 11·6	0·8633	1·8411
,, 13	3 41 32	24 42·7	0·8597	
,, 17	3 38 1	24 11·5	0·8597	1·8459
,, 21	3 34 31	23 38·7	0·8634	
,, 25	3 31 9	23 5·0	0·8708	1·8520
,, 29	3 28 2	22 31·2	0·8819	
Dec. 3	3 25 17	21 58·1	0·8965	1·8593
,, 7	3 22 58	21 26·5	0·9146	
,, 11	3 21 11	20 56·7	0·9360	1·8677

Iris will be in perihelion October 14·7, G.M.T., and nearest to the earth on November 15, her distance at this time being 0·859 (the earth's mean distance from the sun being taken as unity). Her intensity of light may be expected to rather exceed that of a star of the seventh magnitude, 6·8m. according to the *Berliner Jahrbuch*.

The Outer Satellite of Mars.—This object is still under observation at the Observatory of Paris. It was also measured again by Mr. Common, of Ealing, with his 18-inch silver-on-glass reflector on September 24, the angle calculated from the elements which have been given in this column differing from the observed angle − 4°. An observation on September 13, by M. Borrelly at Marseilles, presumed to apply to the satellite, must refer to a faint star, the satellite at the time being in the opposite quadrant.

Binary Stars.—Dr. Doberck, of Markree Observatory, continues his investigations on the orbits of the revolving double stars. In No. 2,156 of the *Astronomische Nachrichten* he has given provisional elements of Σ 1768 and Σ 3121, the latter of which appears to be an object of special interest from the shortness of the period of revolution, which hardly exceeds that of the well-known binary, ζ Herculis. Also elements of Σ 3062, a star which was the subject of a pretty complete calculation by Dr. Schur in 1867. The results of the two discussions are as follow:—

	Schur 1867	Doberck 1877
Passage of peri-astre	1835·196	1834·88
Node	32° 10′	38° 35′
Angle between the lines of nodes and apsides	97° 31′	92° 7′
Inclination	29° 58′	32° 11′
Eccentricity	0·5009	0·4612
Semi-axis major	1″·310	1″·270
Period of revolution	112·64 years	104·415 years

These orbits are in very satisfactory confirmation of each other.

May we hope that at no distant period Dr. Doberck may find he has sufficient material to induce him to investigate the elements of α Centauri; a fair approximation to the true orbit might be expected from his experienced hand.

PROF. ADAMS ON LEVERRIER'S PLANETARY THEORIES[1]

II.

THE nineteenth chapter of M. Leverrier's researches which forms the first part of the eleventh volume of the *Annals of the Paris Observatory*, contains the determination of the secular variations of the elements of the orbits of the four planets, Jupiter, Saturn, Uranus, and Neptune.

In the first place are collected the differential formulæ which are established in the previous chapter, and which give the rates of secular change of the various elements at any epoch in terms of the elements themselves, which by the previous operations have been cleared of all periodic inequalities.

The terms of different orders which enter into these formulæ are carefully distinguished.

If we were to confine our attention to the terms of the first degree with respect to the excentricities and inclinations of the orbits, and of the first order with respect to the masses, the differential equations which determine the secular variations would become linear, and their general integrals might be found, so as to give the values of the several elements for an indefinite period.

In the present case, however, the terms of higher orders are far too important to be neglected, and when these are taken into account the equations become so complicated as to render it hopeless to attempt to determine their general integrals.

Fortunately, however, these are not needed for the actual requirements of astronomy, and for any definite period the simultaneous integrals may be determined with any degree of accuracy that may be desired by the method of quadratures.

In this way M. Leverrier has determined the values of the elements for a period of 2,000 years, starting from 1850, at successive intervals of 500 years. The first steps in this integration were attended with some difficulties, because the determination of the numerical values of the rates of change of the several elements at the various epochs depends on the elements themselves which are to be determined. Hence several approximations were necessary in order to obtain the requisite precision.

After this work of M. Leverrier, however, the extension of the investigation to other epochs, past or future, is no longer attended with the same difficulties. In fact, from his results we may at once find, by the method of differences, very approximate values of the elements at an epoch 500 years earlier or later than those which he has considered. His general formulæ will then give the rates of change of the several elements at the epoch in question, and having these we can determine by a direct calculation the small corrections which should be applied to the approximate values of the elements first found.

This process may evidently be repeated as often as we choose.

It is important to remark that in the formulæ which give the rates of change of each of the elements at the five principal epochs considered, as well as in those which give the total variations of the elements at the same epochs, the masses of the several planets appear in an indeterminate form, so that it may be at once seen what part of the variation of any element is due to the action of each of the planets, and what changes would be produced in the value of any element at any epoch by any changes in the assumed values of the masses.

Consequently, when the astronomer of the future, say of 2,000 years hence, has determined the values of the elements of the planetary orbits corresponding to that epoch, it will be easy for him, by comparing those values with the general expressions given by M. Leverrier, to determine with the greatest precision the actual values of the masses, provided that all the disturbing bodies are known; and should there be any unknown disturbing causes, their existence would be indicated by the inconsistency of the values of the masses which would be found from the different equations of condition.

By means of the work which has just been described, everything has been prepared which is required for the treatment of the theories of the several planets.

The remainder of the eleventh volume of the *Annals* is accordingly occupied by the complete theories of Jupiter and Saturn, the former theory being given in Chapter 20, and the latter in Chapter 21 of M. Leverrier's researches.

The coefficients of the periodic inequalities of the mean longitudes and of the elements of the orbits are not only exhibited in a general form, but are also calculated numerically for the five principal epochs considered in Chapter 19 of these researches, viz, for 1850, 2350, 2850, 3350, and 3850.

The long inequalities of the second order with respect to the masses, depending on twice the mean motion of Jupiter plus three times the mean motion of Uranus minus six times the mean motion of Saturn, are also determined in a similar form.

Chapter 22 of M. Leverrier's researches, forming the first part of the 12th volume of the "Annals," contains the comparison of the theory of Jupiter with the observations, the deduction of the definitive corrections of the elements therefrom, and finally the resulting tables of the motion of Jupiter. The observations employed are the Greenwich observations from 1750 to 1830 and from 1836 to 1869 together with the Paris observations from 1837 to 1867.

To the results given in the Astronomer-Royal's "Reduction of the Greenwich Observations of Planets from 1750 to 1830," M. Leverrier has applied the corrections which he has found to be required by his own reduction of Bradley's observations of stars and his redetermination of the Right Ascensions of the fundamental stars, published in the second volume of the "Annals" (Chapter 10).

The equations of condition in longitude, for finding the corrections of the elements and of the assumed mass of Saturn, are divided into two series corresponding to the observations made from 1750 to 1830, and into two other series corresponding to the observations made from 1836 to 1869. Moreover in each of these series the equations are subdivided into eight groups, corresponding to the distances of the planet from its perihelion, 0° to 45°, 45° to 90°, and so on. From these are formed four final equations, the solution of which gives the corrections of the epoch, of the mean motion, of the excentricity, and of the longitude of the perihelion, in terms of the correction required by the mass of Saturn, which is left in an indeterminate form. The substitution of these expressions in the thirty-two normal equations corresponding to the several groups above-mentioned, gives the residual differences between theory and observation in terms of the correction of the mass of Saturn. No conclusion can be drawn from the ancient observations; but from the modern observations M. Leverrier finds that the mass of Saturn assumed—which is that of Bouvard—should be diminished by about its $\frac{1}{200}$th part. This correction is very small, but M. Leverrier regards it as well established.

On the other hand, Bessel's value of the mass of Saturn, founded on his observations of the Huyghenian satellite, exceeds Bouvard's by about its $\frac{1}{350}$th part.

[1] Continued from p. 464.

The equations of condition in latitude are treated in a similar manner, being grouped according to the distances of the planet from its ascending node. From these equations the corrections of the inclination of the orbit and longitude of the node are found separately from the ancient and from the modern observations. The results differ very little, but the second solution is employed in the construction of the tables. After the application of these corrections to the elements, the agreement between theory and observation may be considered perfect: so that the action of the minor planets on Jupiter appears to be insensible, and there is no indication of any unknown disturbing causes.

There are some peculiarities in the mode of tabulating the perturbations caused by the action of Saturn. The perturbations of longitude and of radius vector are not, as usual, exhibited directly, but instead of them M. Leverrier gives the perturbations, both secular and periodic, of the mean longitude, of the longitude of the perihelion, of the excentricity, and of the semi-axis major of the orbit, and then from the elements corrected by these perturbations he derives the disturbed longitude and radius vector by the ordinary formulæ of elliptic motion.

Where the perturbations are large M. Leverrier considers this preferable to the ordinary method of proceeding. The perturbations of latitude being small, he applies to the inclination and longitude of the node their secular variations alone, and then determines directly the periodic inequalities of latitude.

All these perturbations, whether of the elements or of the latitude, are developed in a series of sines and cosines of multiples of the mean longitude of Saturn, including a constant term, the coefficients multiplying these several terms being functions of the mean elongation of Saturn from Jupiter, which for a given elongation are developed in powers of the time reckoned from the epoch 1850. These coefficients only are tabulated with the mean elongation as the argument, and the perturbations are thence calculated by means of the ordinary trigonometrical tables. The intervals of the argument are so small, that the requisite interpolations are very simple, and the coefficients which relate to the four elements, and depend on the same argument, are given at the same opening of the tables.

The tables have been calculated specially for the 500 years included between the years 1850 and 2350. Nevertheless they may be applied to epochs anterior to 1850, by simply changing the sign of the time reckoned from 1850. For one or two centuries before 1850 this extension will have all the rigour of modern observations, while for still earlier times the accuracy of the tables will greatly surpass that of the observations which we have to compare with them.

M. Leverrier's Tables of Jupiter are now employed in the computations of the *Nautical Almanac*, beginning with the year 1878.

The thirteenth volume of the *Annals* is devoted to the theories of Uranus and Neptune. These theories are not unattended with difficulties. In the first place, these planets are disturbed by the actions of the two great masses Jupiter and Saturn, interior to their orbits, and these actions are modified by the great inequalities of Jupiter and Saturn depending on five times the mean motion of Saturn minus twice the mean motion of Jupiter. In the next place twice the mean motion of Neptune differs very little from the mean motion of Uranus, and thus arise inequalities of long period in the elements of their orbits which are large enough to produce very sensible terms of the second order. Lastly, the mean elliptic elements of the two planets are not yet sufficiently well known.

In a preliminary chapter, the 24th, M. Leverrier investigates formulæ which are specially applicable to the case of a planet disturbed by another which is considerably nearer to the sun. In this case it is easily seen that, by the direct action of the disturbing planet on the sun, perturbations of large amount may be produced in the *elements* of the orbit of the disturbed planet, while the corresponding perturbations of the co-ordinates of the planet are comparatively small. Hence arises the advantage of considering this case apart.

We have seen how closely the theories of Jupiter and Saturn are related to each other. In a similar manner the theories of Uranus and Neptune are also closely related in consequence of the great perturbations introduced into the elements of their orbits by the near approach to commensurability in their mean motions. Hence, before entering upon the separate theories, M. Leverrier devotes Chapter 25 of his researches to the determination of the mutual actions of Uranus and Neptune, and this forms the base of the theories of both planets. The method employed is similar to that adopted in the case of Jupiter and Saturn, and the results are exhibited in the same general form.

It is important to remark that the elements of Uranus and Neptune as determined from observations severally differ from their mean elliptic values by the amount of their perturbations of long period corresponding to the mean epoch of the observations. The apparent elements of Uranus and Neptune for the epoch 1850 have been carefully determined by Prof. Newcomb in his excellent work on the theory of those planets which obtained the Society's medal in 1874. By the application of his own general formulæ, M. Leverrier deduces from these elements the values of the mean elliptic elements corresponding to the same epoch. It may be remarked that the mean elements thus determined will depend on the assumed masses of the two planets, and will therefore require small corrections when more accurate values of the masses have been obtained.

When the secular variations of Uranus and Neptune given in Chapter 19 were found, the elements were less accurately known, and M. Leverrier has therefore recalculated the values of the excentricities and longitudes of the perihelia of the two planets for the same five epochs as before, starting from the mean elliptic values of the elements above referred to.

Chapter 26 contains the completion of the theory of Uranus. The last chapter, which contains the completion of the theory of Neptune, is not yet printed.

The twenty-third chapter also, which contains the comparison of the theory of Saturn with observations, together with the tables of the planet, and which will form the latter part of the twelfth volume of the *Annals*, is not yet printed. The results of this comparison of the theory with observations have, however, been fully published in the *Comptes Rendus*, and I understand that the tables will be used for computing the place of Saturn in the forthcoming volume of the *Nautical Almanac*.

Although the comparison of the theory of Saturn with observations shows in general a satisfactory accordance, there occur some discrepancies in individual years which are larger than might be desired.

During the thirty-two years over which the modern observations extend, viz., from 1837 to 1869, the discrepancy between theory and observation, however, remains constantly less than 2″·5 of arc, excepting in two instances, viz., in the years 1839 and 1844, when the differences amount to 4″·5 of arc.

In the ancient observations only, made in the time of Maskelyne, rather larger differences occur, amounting in two instances to nearly 9″ of arc.

In order to test whether these discrepancies could be due to any imperfections in the theory, M. Leverrier has not shrunk from the immense labour of forming a second theory of the planet independent of the former, employing methods of interpolation instead of the analytical developments. I learn directly from M. Leverrier that this

second investigation entirely confirms the accuracy of the first as regards the periodic inequalities, but that the secular variations of the excentricity and longitude of the perihelion are slightly changed. The effect of these changes is to bring the theory into very satisfactory accordance with the observations of Bradley, but the discrepancies above mentioned in the time of Maskelyne and in the modern observations still remain unaffected. The character of the discrepancies shown by the modern observations makes it very improbable that they can be due to any errors in the theory.

In fact, the error appears to change almost suddenly from a positive one of 4″·4 in 1839 to a negative one of 5″·0 in 1844, a variation of nearly 9″·5 in five years. Now no terms or group of terms due to the action of the planets could thus suddenly disturb the motion in five years, at a given epoch, and then leave the motion unaffected during the following twenty-five years. M. Leverrier is therefore inclined to think that the discrepancies arise from errors in the observations, notwithstanding that the Greenwich and Paris observations are mutually confirmatory of each other.

He suggests that it is possible that the varying aspects presented at different times by the ring may affect the accuracy of the observations of the planet, and may cause changes in the personal equations of the observers, which, from being rather large in the case of the ancient observations, have gone on diminishing as the system of observation has become more perfect.

One unlooked-for result follows from M. Leverrier's comparison of his theory of Saturn with the observations. Considering that the influence of Jupiter on the longitude of Saturn may amount to 3800″, it might have been expected that from observations of the planet extending over 120 years the mass of Jupiter could have been determined with great precision. M. Leverrier has found, however, that this is not the case.

The equations of condition furnished by the comparison of the heliocentric longitudes of Saturn as deduced from theory and observation contain five unknown quantities, viz., the corrections of the assumed values of four elements and the correction of the assumed mass of Jupiter. On solving the equations with respect to the first four unknown quantities, the corrections to be applied to the elements are found to be greatly influenced by the intermediate correction of the mass of Jupiter, and after they have been substituted in the equations of condition, the coefficients of the correction of the mass of Jupiter in great part destroy each other, nowhere amounting in the resulting equations to one-tenth part of their values in the primitive equations. Hence these equations are insufficient to determine the mass of Jupiter with any precision. Consequently, in the formation of the Tables of Saturn, M. Leverrier has employed the value of the mass of Jupiter determined by the Astronomer-Royal from his observations of the 4th satellite.

The result which has just been noticed will appear to be less paradoxical if we consider that by far the larger part of the disturbances which Jupiter produces in the motion of Saturn is represented by the inequalities of long period which affect the mean longitude and the elements of the orbit. Now in the course of 120 years these inequalities have run through only a small part of their whole period, and therefore, during this interval, the greater part of their effects may be represented by applying changes to the several mean elements equal to the mean value of the corresponding long inequalities during the interval. It is only from the residual disturbances, which are comparatively small in amount, that any data can be obtained for the correction of the mass of Jupiter.

In the course of a few centuries, when these long inequalities, as well as the secular variations of the elements of Saturn, shall have had time to develop themselves, it will be possible to determine the mass of Jupiter from them with all desirable precision.

THE GLANDULAR ORIGIN OF CONTAGIOUS DISEASES[1]

TEN years ago, on the occasion of a Congress held in this town to discuss the question of the disposal of sewage, I had the honour, at the request of the committee of management, to deliver a lecture on the subject of the poisons of the spreading or communicable diseases. An abstract of the lecture was afterwards printed by the Congress, and for a time it gained a wide circulation.

The lecture of which I speak was based on a series of experimental researches which for some years previously I had been carrying out on the question of the mode of production and communication of those diseases which were anciently called plagues or pestilences, but which are now called communicable or spreading diseases.

I do not think that at a health congress like the present I can do better than recall attention to this same subject. The suppression of plagues is one of the grandest and supremest efforts of the sanitary reformer. The suppression can never be accomplished until all educated people understand the advances of modern science as to the cause and mode of origin and mode of propagation of these diseases. Whatever, therefore, tends to strike out light of knowledge on these subjects tends to elucidate, and though the spark lighted may go out again it may help to show the way.

I shall in this present effort first go back to the point where I stood when here ten years ago. I shall then briefly survey the course of thought that has sprung up between that time and the present. Next I shall state the position of my own views as influenced by the work of the past ten years. Lastly, I shall touch for a moment or two on the practical applications of theory to the development of practice.

Outline of the Glandular Theory.

From my researches previous to the year 1867, and which formed the subject-matter of my previous lecture here, I had discovered that the fluids secreted during various stages of disease in some forms of communicable disease could be made to propagate disease. A portion of secreted fluid taken from a patient of Mr. Spencer Wells, a patient who was suffering from surgical fever following upon the operation of ovariotomy, had been made to produce a definite form of fever in an inferior animal by being simply brought into contact with the peritoneal surface of the animal. The secretion from the peritoneum of the affected animal was shown by further experiments to have the power of inducing the same order of phenomena of disease in other similar animals, and through four generations of animals the phenomena were repeated. These were the first experiments in which this class of phenomena of disease by direct propagation and repropagation were produced synthetically. They have since been performed and modified in many ways, and the origination of them has been assigned to different experimentalists, but I am entitled to say they were the first of the kind; they were carried out in the years 1864-65, and they were communicated to the Association of Medical Officers of Health in the year 1865.

During the same course of research I made an attempt to separate the poisonous matter from the poisonous secretion, and in one attempt of the sort I believed myself to have been successful. Certainly I separated a substance which was exceedingly poisonous in its action, and which, after the manner of an alkaloid, combined with

[1] A Theory as to the Natural or Glandular Origin of the Contagious Diseases. Address by the president, Benjamin W. Richardson, M.D., F.R.S., at the Sanitary Congress, Leamington, October 3.

acids producing salts which were not only themselves poisonous but which reproduced poisonous secretion. In the lecture delivered in this place in 1867 specimens of these salts and of the substance from which they were derived were placed on the table.

To the poisonous substance, that is to say, to the base of the poisonous matter of the communicable diseases, I gave originally the name of *septine*, and I classified all diseases that are induced by such substance, *septinous diseases*. Before this period the diseases had been named *zymotic*, under the idea that they were connected with a process of zymosis or fermentation. They are still commonly known by that name. Since the name I suggested was given to them, they have been called septic diseases, and the term septicæmia has been brought into use in relation to them. I am of opinion, with all respect, that the word septine, as applied to the basic poisonous matter, and the term septinous, as applied to the phenomena, are the two simplest and best terms we can employ.

As the inquiries which led up to the experiments with septine progressed, I was led to form a view as to the nature of the poisonous base and as to its mode of origin. As I have already said it seemed to me to be an alkaloid, or chemical substance resembling in physical properties morphine, strychnine, and other bodies of that class, but derived, not like them from vegetable, but from animal organic matter. The difficulty in proving this lay in finding a reason for the various effects of the septinous material. If it were a common base like that which I suspected I had found, why should it not always produce the same form of septinous disease? Why should it, on the other hand, produce, as we know it does, many kinds of disease, each having a certain general likeness to the others, but each at the same time different in many important details, as different, for example, as small-pox is from scarlet fever, or measles from hydrophobia?

The difficulty in this way suggested led me to reflect on the connection which might exist between the bases of the different secretions of the animal body and the matter of septine. Each secretion yields some organic product; the gastric secretion pepsine, the salivary secretion ptyaline, and so on, and each secretion plays a different part in function although the organic bases of them all may present a general similitude of construction.

Thereupon I was led to the conception that the secretions of the animal body are in fact the sources of the septinous diseases, and that the various septinous diseases are, in fact, all of glandular origin; that in every case of disease the poison producing it is nothing more and nothing less than a modified form of one or other secretion.

In the lecture of 1867 delivered here a sketch was supplied of the number of diseases which affect the human family. They were stated to be about two hundred and fourteen in number, that is to say, when we classify the symptoms together so as to make them into great groups to which we can give specific names, we may reckon up two hundred and fourteen such groups of diseases. Amongst these groups I described one group as depending for its cause on the action of organic poisons.

The diseases produced by the organic poisons were classed as follows:—

Small Pox.
Measles.
Scarlet Fever.
Diphtheria.
Typhus Fever.
Typhoid Fever.
Erysipelas.
Hospital Fever.
Puerperal Fever (or the fever which occurs to women in child-bed).
Cholera.
Yellow Fever.
Ague.
Glanders.
Boil and Carbuncle.
Infectious Ophthalmia.

On the nature of the organic poisons which produce the diseases I urged the following points:—

(*a*) That in every case the poisons are in themselves specific. Each poison has a specific property, always bringing out the same disease through countless ages. From the time when man was first attacked by them, on to the present time, I have no doubt that each of the communicable diseases has been developed from, and has depended upon one specific poison.

(*b*) That the organic poisons are inodorous, have no smell whatever, and that no communicable disease ever depends upon the mere gases of decomposition of organic matter.

(*c*) That as regards the organic poisons themselves and their physical properties the great type of them all is represented by the poison of any venomous snake. If we puncture the poison bag, there exudes from it a fluid substance that contains the poison. If we gently dry that down, it becomes a darkish, somewhat powdery, half-glistening mass. It is the type of all the poisons which produce disease.

(*d*) That the special poisons are separable, and that I had separated one of them, namely, the poison of hospital fever. This is a secretion formed in the wound of a person suffering from surgical injury, and as it could be obtained in large quantities, it had been specially selected for the purpose of experiment. The poison, when obtained in large quantities, could be evaporated to the form of an extract or syrup, and produced, when dried, a substance resembling closely the snake poison. It admitted of being pulverised, and when introduced into the wound of a healthy animal, it produced symptoms similar to those of the patient from which it was taken.

A specimen of the poison of hospital fever, so prepared was shown. It was extracted from the fluid of the peritoneal cavity of a lady who had been operated upon for ovarian disease by Mr. Spencer Wells.

(*e*) That the poison thus obtained may be introduced into the body in various ways; that communicated to an animal, it will give to the body of that animal the same poisonous property as was possessed by the poisonous substance first introduced; the poison, that is to say, could be passed on, and made to affect another animal, and so through a series of subjects.

(*f*) That in the course of some diseases, these poisons are separated by nature in an almost pure state. This is singularly the case with regard to the poison of small-pox. The poison of small-pox may escape from the surface of the body, in an early stage, as a very fine vapour, and in that way communicate disease. It may be communicated in a fluid form, as we know when we use it by inoculation. In a dry state, as in the scale of a small-pox patient, it is innocuous till it comes into contact with the water or with the fluids of the body; then it becomes poisonous.

(*g*) That the poisons will probably dry solid. In the solid state they are inert, but they are capable of re-absorbing water apparently after any lapse of time, and of regaining their activity.

(*h*) That they admit of being charged with water almost to any degree; but that as we progress in charging them with water, and diluting them with water, they entirely lose their active power. This accounts for a fact which was observed by the famous Dr. Fordyce in the last century. At that time inoculation for small-pox was the rule; and Dr. Fordyce thought, "if he diluted the poison he would produce a milder form of disease." In fact, he was aiming in this way to produce what Jenner afterwards did produce by vaccination, namely, a modified small-pox. He took the poison of small-pox, mixed it with water, and refined it to a considerable extent, and he inoculated patients with the diluted solution. He then found out the fact—that, up to a given point, dilution made no difference, the poison always producing the disease; but beyond that certain point of dilution there was no disease at all produced by the solution—not even a milder disease. This was in accordance with my experiments,

from which I found that the organic poisons retain their activity up to a given point of solution, and beyond that the water renders them inert. Through their extreme capacity for becoming watery, they lose their activity altogether.

(*i*) That the poisons are transferable also by the vapour of water, and in this way may escape from the living body. So long as a person is affected with these poisons, and is giving off vapour at a certain temperature, he is poisonous. The poison is distributed by the vapour, and the vapour is diffused in what I might almost call invisible spray. The poisons are mechanically carried with the vapour, and the vapour from the affected person may be absorbed by the healthy person. But as soon as the body is dead, the vaporisation having ceased, or a reverse process having been set up,—that is to say, there being a condensation of vapour as there sometimes is on the dead body,—the poisons are no longer infectious in the ordinary sense of the word.

(*j*) That the poisons are harmless in their dry state, but commence to resume their activity in water. In order to ensure their continuous action, they need certain temperatures—certain degrees of heat; that in this respect one poison often differs materially from another; and that this marks out on the surface of the earth a specific range for some poisons. For instance, the poison of typhus fever is probably volatile, and condenses with difficulty, with the result that it only lives at a given low temperature, and that at a certain degree north of the equatorial line, the disease ceases. There are other poisons which require a greater degree of heat for their distribution, of which the poison of yellow fever is an example. If yellow fever be brought from a hot country to one of our northern ports, it will not live. It may linger for a few days, but as a rule, it will not extend.

(*k*) That the poisons are all capable of being destroyed by various means. They are all destroyed, as already said, by extreme dilution. They are all destroyed also by heat and by oxidising agents. The mere exposure of them to moist oxygen destroys them rapidly. The exposure of them to ozonised, or electric oxygen, destroys them even more rapidly than ordinary moist oxygen. Exposure of them to chlorine is instantaneous destruction to them. Exposure to iodine is nearly as effective, and if the iodine can be diffused equally, it is as destructive as chlorine. Exposure to bromine leads to the same result. Exposure to nitrous acid has the same kind of effect, but not in so marked a degree. Exposure to sulphurous acid likewise produces destruction.

(*l*) That snake poison is destroyed by sunlight, and that the destruction does not depend upon the temperature. That bright sunlight is probably one of the means for destroying the organic poisons.

(*m*) That almost all the organic poisons are preservable by cold, and that, in fact, there is no limit to the preservation of them by extreme cold. The poisons are preserved also by many antiseptics. Sulphur, creasote, and arsenic, hold these organic poisons in preservation, so that they preserve their active properties.

(*n*) That some of the poisons are only poisonous during certain stages of their decomposition; with regard to the disease called hospital-fever, there is perhaps only one certain stage when the secretions really contain the poison. There is a certain given stage in the process of the manufacture of the poisons when the secretions change, and at that point the poisonous matter becomes innocuous.

(*o*) That in considering the development of these poisons it is a common error to suppose that they multiply from a germ as offspring multiply from parents, but that what occurs is this:—Each particle of any one of these poisons brought into contact either with the blood of the living animal or with certain secretions of the living animal, possesses the property of turning the albuminous part of that same blood or that same secretion into substance like itself. The process of change is catalytic. It is a change by which a body is transformed by the presence of some other body which does not itself undergo change. The multiplication of the poison thus takes place through the force of secretion of the person affected, not through the propagation of germ from germ. For instance if poison producing contagious ophthalmia be passed from the eye of one person into the eye of another, presently there is a free secretion. The secretion soon is profuse and is affected by catalysis from the poison. If the inoculation has been deep the whole animal will be affected; if it has not been deep only the eye will at first be affected. It is not that the particle of poison has propagated a new particle, but it is that the natural secretion of the eyeball has come in contact with a speck of poisonous matter, and immediately at that point where the speck of poison was there is a change in the secretion. This process widens the circle, more secretion pours out and more poison is produced, and the increase goes on until in the end the whole body of the animal may become affected by absorption into the body from the injured surface of poisonous matter: ultimately, *i.e.*, the poison is absorbed into the blood.

(*p*) That as a general rule the human body furnishes all the poisons that the human body suffers from, that is to say, there is a progression of poison from one body to another, and that ordinary secretions may change and become poisonous without previous infection. This, I showed, had been remarkably brought out in the case of puerperal poison, where a secretion from the hand of the accoucheur had produced puerperal fever. In the case of peritonitis, or inflammation of the peritoneum, there is a secretion which may be carried on the hand of a healthy person and reproduce the disease. Typhus may be produced by the overcrowding of persons in a room through the vaporisation of organic matter at a low temperature. Thus we may have springing up *de novo* an organic poison which afterwards, on being introduced into one particular body, becomes increased by the secretions of that body.

(*q*) That as regards the mode in which the organic poisons may be transmitted, they may travel in each of three ways. They may travel by means of sewage as dry solid matter; and all the poisons do this constantly. They may be wafted in the air, or carried by means of linen saturated with the secretions of patients and dried. Again, they may travel in water or in water suspended in the form of vapour.

(*r*) That the mode of the entrance of organic poison into the body, when they enter by contact, varies with the different poisons, the character of the poison changing the mode of its introduction. The poisons of measles, scarlet fever, and typhus are inhaled. The poisons of small-pox, diphtheria, glanders, erysipelas, hospital-fever, and ophthalmia, require direct contact. The poisons of cholera, yellow fever, and typhoid fever seem always to be swallowed poisons, and may be called, specifically, the poisons of sewage, and therefore mostly travel in a fluid form. They may, nevertheless, travel for short distances as fine dust, and they may travel in water in the form of vapour.

The thought that the poisons of the various spreading diseases are poisonous secretions, and nothing more, came naturally out of my researches. I realised, as it seemed to me, that all these spreading and communicable diseases spring out of the living animal body. That they are as distinctly the offspring of living animals as real progeny are, and that to look to outside sources for them, to look to vegetative growth for them, for example, or seedling, is merely to ignore the basic facts which lie obviously before us for lesson and learning. As well suppose that procreation of animals is due to an external vegetable product or other product dissevered altogether in its origin from the animal as that the poison which creates disease of a communicable kind is in such manner dissevered as to its origin.

Another thought which occurred to me in the course of my labours, and which I expressed in those earliest records of them, has relation to the force by which the poisons of the various diseases are developed and thrown off. It is well known that the production of the poisons in a living body infected by one of them is limited in respect to duration of time of production even when the body lives and recovers. This fact seemed to me to prove to demonstration that the poison itself is produced by the affected body, and is determined in its production by some natural function of the body or of some part of it. On the basis of my theory, that the poison in every case is a modified secretion, this view of the force of production of the secretion is easily accepted as in accordance with natural law. The force of production is the force of secretion, and so long as the secretion continues changed in character, so long it is thrown off as a poisonous secretion. But so soon as the modification of secretion which rendered it poisonous is stopped, so soon the secretion, flowing onward as before, is rendered innocuous, that is to say, no longer poisonous. If this were not the case, there is no reason, as far as I could see, why, in every instance of infection the infected person should not die. Endow the poison itself with independent forces of life and of reproduction, give to it a distinct reproductive life of its own; then why should it ever cease to reproduce? Why should it not in every case continue to increase within the infected body indefinitely until it kills the body, and why should any one ever recover? But consider the poison as a part only of the animal body itself, a substance to be eliminated from the body by natural methods and then the process of removal of the poisonous condition comes into the natural course of events, and recovery is a natural process, unless some unusual conditions occur to interrupt the natural course.

We see in a common nasal catarrh the outline of this scheme. There is first a dryness of the secreting surface, with reflex nervous irritation and much nervous depression and disturbance thereupon in the circulation of the blood. After a time there is a copious secretion from the nostrils, which continues until the disturbed nervous balance is brought back to steady natural action. At that time the excess of secretion is checked, and nothing more is left than the local effects of hardened secretion or scale due to the desquamation caused by the excessive previous action. In outline this is really the natural course of every epidemic disease, with the exception that the secretion of a catarrh is not definitely proved to be a contagious secretion. I believe it may be so, and sometimes is so; but I need not press the point. The illustration is adduced merely to show that the course of the disease is from within outwards, and that it is checked in its course by restoration of internal natural function. If catarrh were produced by some external vital agency, reproductive in character, lighting upon the nasal tract; if it were due to the colonisation of the nasal tract by an army of foreign invaders which settled there and began and continued to replenish and multiply, when would the catarrh cease? It would, as far as I can see, continue, until by destruction of parts and continued abstraction of secretion and extension of mischief over a wider tract of surface, it killed inevitably.

A catarrh, according to my view, as it was originally expressed, is typical of all the diseases which run a given course, and are called spreading diseases. It springs up constantly from external atmospheric variations; it runs a given course; it subsides. It is an epidemic, and it would be a true contagious epidemic if the matter secreted from the nasal cavity and the conjunctiva were not so innocuous. As I have hinted already, I believe it may be contagious. I am quite sure that many times in my life I have taken catarrh by coming near to a person who was affected by it, but whether this contagion is sympathetic or toxic, I am not able to define. On these intimate relations I shall have more to say on a future page.

The Germ Theory.

In the ten years that have passed since the time named, another hypothesis in reference to the spreading diseases and in relation to their origin from particular poisons has been brought prominently forward. Owing chiefly to the simple name which has been given to this hypothesis, and the commonness of the analogies on which it is based, it has gained much popular favour—I need hardly say that I refer to the so-called germ theory of disease.

This hypothesis has been most prominent for eight or ten years, but it is really a very old speculation indeed, perhaps one of the oldest in medicine. It has its root in the fancy of the analogy that as seed cast on the ground yields, or may yield, a certain harvest after its kind, as a field or garden plot may become fertilised by vegetable seeds or germs which may come to it borne by the atmosphere or by other modes of conveyance, so the body may be infected with germs of disease, which germs, being received in the body as a field for their reception, may increase and multiply in the body, and by their presence excite the phenomena which particularise all the special diseases of a communicable kind.

In modern times Dr. Grove, late of Wandsworth, was the first to advocate this hypothesis, and I need not tell a learned assembly like the present that it has been most energetically advocated more recently by many of the ablest foreign and English men of science. In the course of the discussions and of the researches which have been conducted on this subject much knowledge has, I am sure, been gained in the domain of natural history, and much interesting discussion for history has been written on the origin of some forms of life. But I protest that the attempt to connect this knowledge with the phenomena of the various communicable diseases, so as to suggest, or, as some do, to assert, that the diseases in question arise from germs, and that the person affected with a contagious disease has been fertilised like a piece of ploughed land or virgin soil by a crop of germs, and that in turn he is the soil in which another crop is being produced by the independent increasing and multiplying of the germs in him, I protest I say, that this hypothesis is the wildest, the most innocent, the most distant from the phenomena it attempts to explain, that ever entered the mind of man to conceive. What most astounds me is that men who are conversant with the practice of physic, who are treating diseases of a communicable kind every day, should for a moment connect such a hypothesis with the phenomena they have under their observation. Does any one of them believe that hydrophobia is from a germ, that syphilis is from a germ, or other diseases I need not specify?

It is suggested by some advocates of the germ speculation that the cause of the communicable diseases is after the manner of the putrefaction of dead organic matter. Does any physician who thinks as he observes, see anything like a general rule of putrefactive change in the contagious diseases? He may of course see local decompositions of secretions and of blood itself in the course of any of the diseases, but these he knows are all secondary results, while he may see and constantly does see all the diseases running their course without any sign whatever of the kind. Nay, in regard to one disease—cholera—he may, as I have done, see it run its fatal course and leave the dead bodies as loth to decompose as if they had been embalmed. Again, does any physician, who thinks as he observes, fail to see that the first symptoms of every one of the contagious diseases are purely nervous symptoms, that they indicate nervous irritation, and that the particular local injuries which occur are not primary at all,

but are dependent on special nervous change modifying nutrition at the part. Between a boil or abscess and a pustule of small-pox what is the difference except in degree of purulent matter formed at one point of formation of matter?

There is nothing whatever in fact in the clinical history of plagues that connects them with the hypothesis of an origin from germs produced without the body and entering it to fertilise it and create a decomposition. When I say there is nothing, I mean there is nothing except the analogy of which I have spoken above, and even that breaks down, for the analogy of the fertilisation of a field by seed means always a definite process of fructification and of results from it; whereas in the history of epidemic plagues there is no such definition.

The germ hypothesis fails, however, on other grounds than the clinical. If it were true that living germs possessing an independent growth and vitality enter the animal body, that every disease of a communicable kind is due to its own external living germ, and that the germs continue to multiply and increase by an independent action of their own; if this be indeed true, why do the germs after a certain time cease to multiply and allow the sick person to recover? Why do they not go on multiplying until the person is infested in every part and fatally stricken? Who would get well from a disease due to living self-propagating contagions? Again, who, if the hypothesis were true, would escape fertilisation? A general fertilising diffusion of self-propagating matter in minute invisible form entering the body as the air may enter could hardly be expected to select a small minority of a population, and if it did so at the first, why should it do so when it had seized upon many centres in which it could increase. But the history of all the communicable diseases shows that each epidemic affects individuals individually at different periods in the course of the epidemic according, as a rule, to exposure to the infected, and that the period of the disease is limited by a development and a course rendered in certain periods of time.

I need hardly add in objecting to this germ hypothesis, because the fact is admitted, that not only has no one ever seen a germ of disease, but that no one has ever traced any order of germination in relation to any of the communicable diseases. When a really living self-propagating thing goes through its phases of life and action, like, for example, the yeast growth, we can trace it through its course of action on organic substances, and can study its effects, the changes it produces and the products of such changes. In the epidemic diseases we have no such guidance, no trace of it. Their phenomena, indeed, are opposed to the idea of the self-action of a foreign vital material.

Later Observations on the Glandular Theory.

I turn again to a brief review of the glandular theory of the origin of the contagious diseases, and of the advances I have made in support of that theory during the period of the past ten years.

In that time I have seen no reason to change my views on the subject of the glandular origin of the communicable diseases. On the contrary, every new observation has tended to confirm it and to make as I think the demonstration of its truth the more definite.

In continuance of observation I have noted that the number of the distinctly communicable diseases is closely related with the number of secretions. The poison of hydrophobia is from the salivary secretion; of diphtheria from the mucous glands of the throat; of scarlet fever I believe from the lymphatic glandular secretion; of glanders from the mucous secretion of the nasal surface; of typhoid from the mucous glands of the intestinal surface; and so on. In some instances the blood itself is infected, and the corpuscular matter becomes the seat of the catalytic change.

A second point which has occurred to me is that the matter or particle which sets up the poisonous action, instead of being living matter, is matter actually dead, and that its effect for evil depends, in fact, upon its being dead. I mean that the dead particles of organic matter in contact with living are the cause of the physical change which transforms the new particles of secretion into poisonous particles as they are brought up to the infected surface to be influenced by the infection.

On the ground that the poisons are always of glandular origin I have been led to the conclusion that under certain influences affecting glandular action the poisons may be made to originate directly through nervous impression without the necessary intervention of an infecting particle. In many epidemics it is common to see a number of examples of the prevailing disease the origin of which is traceable only to fear or anxiety. We call these nervous cases, and we try to define them as such and as distinct from cases due to contagion of a direct kind. But the symptoms are the same as those which follow actual contagion, and in epidemics of cholera they take even a fatal character. My theory explains fully the reason of this. It indicates that an extreme nervous impression acts on the glandular nervous supply, paralyses the glandular function, and thereupon produces the same phenomena as is produced in other instances by the action of a specific poison.

The theory in this manner accounts for the origin of an epidemic disease from an impression made on the nervous system without the direct contact of poisonous matter, as well as for the after-propagation of the disease by distribution of poisonous particles when that is communicated from an infected to a healthy person. It accounts equally well for the production of disease and of a poisonous glandular product under conditions of starvation and cold, by which the nervous tension is reduced. Again, it accounts for the production of disease and of a poisonous glandular secretion under special atmospherical conditions in which the activity of the atmospheric oxygen is reduced in sustaining power.

It has occurred to me further, as a result of the study of the action of the poisonous particles, that when they are brought into contact with the secreting surface, their action towards the body at large is, in the first instance, directly on the nervous fibre. The poisons act in the first instance as irritants on the peripheral nervous surface, and their effect may, I believe, extend particle by particle, as by diffusion, through the whole length of the nervous cord to the nervous centre. I have no doubt this is what slowly takes place in hydrophobia. I believe this is what takes place in diphtheria when paralysis is the sequel of the acute symptoms of the malady. I believe the same mode of progression of the poisonous influence is what happens after inoculation with matter of small-pox, that the severe nervous symptoms which mark the onset of that disease are due to the extensive injury inflicted on the nervous organisation, and that the diffusion of the eruption over so wide a surface of the skin and mucous membrane is the reflex on the peripheral nervous surface from the nervous centres.

It is worthy of special notice in connection with this part of my subject, that in the communicable diseases attended with an eruption on the skin or nervous surface, the eruption, as a rule, takes a circular form. If it be a point of vascular blush, a patechial spot, it is a rounded spot; if it be a pustule it is rounded; if it be a more diffused rash it commences in centres which are rounded points. This appearance is an indication of nervous injury. The rounded surface is the radius of injury done to the nervous supply of that part. It is a paralysis of the centre of nervous distribution over the affected part. My researches on the influence of extreme cold on nervous

function is strikingly illustrative of this fact. They suggest that the nervous impressions sent from the centres to the peripheral surface when they reach the peripheral surface, as on the surface of the skin, spread out like circling waves, just, in fact, as water spreads out in circles on a pool when a stone is made to impinge on the surface.

Some other recent investigations on the mode of action of the poisons of the communicable diseases has led me to suspect the source of the symptom which is so common to most of them, and which is known as the attendant fever. The fever is of three kinds: primary, reactive, and remittent. The primary fever is that which precedes and attends the eruption of an eruptive disease. The reactive fever is that which succeeds the extreme collapse of an acutely-exhaustive disease like cholera, or an exposure to extreme cold. The remittent fever is that which succeeds upon an acute form of disease, and indicates either that there has been secondary absorption of matter from an abraded surface in contact with poisonous substance, or that some fibrinous or pustular matter has formed within the body, as it were, and become a new and permanent centre of infection.

The first of these forms of fever is, I believe, due to the impression on the nervous centres by the poison in the manner I have described above.

The second, the reactive fever, is, I believe, due to the same action as that which locally may be induced by extreme cold, viz., by an influx of blood into vessels that have been paralysed, and by a rapid radiation of heat from extensive surface of blood.

The third form of fever, the remittent, has an origin, I believe, specifically its own. I have found that pustular matter and all secretions containing fibrinous or cellular structure have the property, by their presence, of liberating oxygen from solution. This extends, as I have found, to blood charged with oxygen, and I am led to the inference that when there is an absorption of such matter into the circulation it causes an undue liberation of oxygen with a quicker combustion, a fever which lasts until the exciting matter is itself destroyed and eliminated, and which does not recur until there is re-absorption of more of the exciting agent. In this physiological mode I should explain all the phenomena of the remittent attack; the cold stage incident to the absorption of the exciting matter; the hot stage incident to the period when, by its presence, the exciting matter is setting free excess of oxygen; the sweating stage when, by rapid elimination through the sweat glands, the equilibrium of temperature is restored.

The study of the glandular theory of the communicable diseases has suggested to me another thought which observation of the diseases fully confirms, viz., that these diseases, like all which have their root in nervous derangement, present a distinct heredity. The impression of disease made on a nervous centre is transmitted. There can be no doubt as to transmission of tendency to particular communicable diseases. Any physician in full practice can find any amount of evidence on the fact by simple natural inquiry. Typhoid fever is clearly a disease possessing hereditary transmissible quality. Diphtheria is the same. Scarlet fever is the same, and small-pox I should suspect was once almost universally so characterised. These facts alone, one of them alone, is sufficient to stamp the origin of the communicable diseases as from the animal body itself. It is certainly one of the best of proofs of the truth of the theory of the glandular origin of the poisons. It will be seen at once by those who look with sufficient patience, that the mode of connection of the diseases in hereditary line is the same as that which connects hereditary qualities of every kind, physical type, mental type, all else that binds many individualities into one family.

Lastly, the study of the glandular theory of the communicable diseases enables me to offer the the most rational explanation of the phenomenon of non-recurrence of the diseases after they have once attacked a person susceptible to them. It is well understood that, as a rule, a person who has been affected by a communicable disease is not affected a second time. To this rule there are many exceptions, but on the whole it holds good. On my theory the reason of the phenomenon is simple enough. They who are susceptible are born with a nervous impression tending to the production of a glandular secretion easily changed into poisonous secretion under the direct action of contact with poisonous matter, or even under the influence of a central nervous depression, whereby the glandular function is deranged. But when such a person has passed through the ordeal the tendency, for a time at least, disappears owing to the complete modification of glandular function that has been induced, to the free elimination that has been established, and probably to the change in the nervous matter itself that has resulted from organic modification. Hence the organism becomes susceptible for a time, and if the tendency be not intense that time may mean the whole of the life. Indeed as life advances and nervous susceptibilities derived directly from ancestry lapse into individual self-sustained susceptibilities, these tendencies to disease subside as a general fact, and lose their activity if not their existence.

I turn, in conclusion, to consider for a moment from the view of the glandular theory of the communicable diseases, the practice that is suggested for the suppression of the plagues of mankind.

It will be seen at once that on this point nothing can be wider than the distance between the idea of contagium as a living self-productive thing, reproductive and independent, and the theory of the production of the contagium in each affected person by the force of production of his own secretion. The latest and one of the ablest advocates of contagium vivum, Dr. W. Roberts, says, respecting contagium:—"We know of nothing that exhibits the phenomena of growth and multiplication except a thing possessed of life."

I admit that readily, but my argument is that the process of secretion is a process of life, and that this living process, perverted as I have described, is amply sufficient to account for the production of all the poisons that exist to cause the communicable diseases, that it accounts for the number of these diseases, and more, that it accounts for the limitation of the number.

The Glandular Theory in its Application to Practical Sanitation.

The practical usefulness of the theory, however, consists in its direct application. If the contagium vivum view be true, if the air around us is charged with invisible germs which come from whence we know not, which have unlimited power to fertilise, which need never cease to fertilise and multiply, what hope is there for the skill of man to overcome these hidden foes? Why on some occasion may not a plague spread over the whole world, and destroy its life universally?

My theory presents an altogether different aspect. I say to living men and women, it is you who are the producers of the communicable diseases, or if it be not you yourselves it is one of your lower earthmates in creation, some domestic animal that shares with you the power of producing a poisonous secretion and of giving a hereditary stamp of production to such poisonous product. I look on the man or animal affected with a contagious disease as one precisely, for the time, in the position of the cobra or other animal that naturally secretes a poison, and recognising this fact I see at once that the danger is all but limited to the person affected.

Isolate that person from the rest of mankind, take care that his secretions, volatile, fluid, or solid, do not come into

contact with the secretions of susceptible healthy persons, and the danger is over. With the recovery of that person, that is to say, with restoration in him of a natural secretive process, the poison is destroyed; or should he unfortunately die, then with the death of his power to produce further secretion the danger is over, unless from his dead body some of the poison formed before the death be actually carried away to infect. In a word, if my theory be true, we sanitarians have complete mastery over the diffusion of the poisons of all the communicable diseases. We have but to keep steadily in view that the producing and reproducing power is in the affected body, and we can, even with our present knowledge, all but completely limit the action to the propagating power of that body—its power, I mean, of secretion and diffusion of secretion.

Beyond this, if the theory be true, we must expect, as we reduce the communicable diseases of one generation to reduce the tendency to them in the next generation, so that in time the heredity to particular spreading disease shall be thoroughly wiped out.

The theory suggests a profitable line of research on the subject of the production and reproduction of some of the poisons by the inferior animals and their transmission in that course to man. It brings all the inferior animals, in respect to their health and comfort, under our especial human care, not only for their sakes, but for our own self-preservation.'

Finally, the theory suggests to those who are engaged in treating diseases of a communicable kind the best means of arresting the progress of a communicable disease even when the phenomena of it have been developed in an individual. It leads us physicians to take a precise view, in each such case, of the nervous and glandular processes that are out of the natural order of work; it suggests to us to seek for remedies amongst chemical agents which affect special secretions; and it shows us how to place the sick under such conditions that the secondary absorption of their own poisonous secretions,—that deep absorption which, according to my experience, is the actual cause of death in the great majority of cases of contagious disease,—may be avoided.

In every direction, in fine, in prevention and in cure, the glandular theory of the origin of the communicable diseases opens practical work and hopeful work.

I have for some time past sought for a favourable opportunity of once more putting forward this theory of the natural origin and cause of the communicable diseases of men and animals. The present is opportune to the fullest degree, and therefore I have seized on it. I am too earnest after search of truth for its own sake, too certain that in science everything false must fall, and everything true must remain, to feel any sense of anxiety as to the fate of my simple theory, by the side of the doctrine of a living contagium. If my doctrine be as true as I believe it to be, it will live, whatever force be arrayed against it. If it be not true, I would be of the first to welcome its end, and to hail the ascendency of what is absolutely provable and certain on the momentous questions that have occupied our attention.

Meantime, I know I could not do a better thing for my own views than submit them once more to the public eye through the audience which has now so attentively listened to the argument.

NOTES

THERE has been a great deal of talk during the last few days, by prominent public men, on the advantages of some equivalent for university education for all the people, an education, too, in which science would be allotted a just place. Last week a Nottingham, the Earl of Carnarvon and Mr. Gladstone said much that was at least true on the advantages of an institution such as that newly founded at Nottingham, and each from his own standpoint lauded the advantages of wide culture for all classes. Both Mr. Gladstone and Mr. Forster on Tuesday at Bradford seemed distinctly to approve of the movement for creating Owens College a University, and the only difficulty now seems to be the question of power to grant degrees. But surely those who are so eager on the latter point forget to distinguish between the shadow and the substance; the question of degrees will no doubt settle itself after the University has been established. Still we hardly sympathise with the trade-mark view of degrees propounded by Dr. Appleton in the *Times*. Bass's or Allsopp's label is imitated because their ales have a high and no doubt well-deserved reputation. But there were good ales before the names of either of the Burton brewers were heard of; there is the fine old Oxford ale, for instance, which, to judge by the public taste, has been improved upon by its new Burton rivals. Mr. Forster, however, we must say, seemed to think Oxford deserving of a word of praise for its present activity. Mr. Forster's address at Bradford was no mere essay on the beauty of culture, but the weighty utterance of a "practical" man who is forced to confess that he daily feels the immense disadvantage of having had no early training in science. He produced himself, in fact, as a practical comment on Sir John Lubbock's previous advocacy of the introduction of science into elementary schools. "His ignorance of science," he said, "his want of having been taught elementary laws of science when a boy, he felt every hour of his life, and it was too late now to learn. Science, if learnt at all, must be learnt in boyhood, and it was really disgraceful that in this civilised country, in this intellectual age, any one should be brought up in ignorance of the laws of nature, upon the breaking or keeping of which depended our happiness, our lives, and almost everything that relates to us. What a loss of pleasure, and what a different world the outside world of nature would be to him, if he could look around and understand the meaning of the various forces which were at work; and there was no doubt that a boy, even at an elementary school, if he learnt the elements there and went on afterwards, would get that kind of knowledge of the laws of science that it would become easy to him. There was a great talk about the dead languages. He was not going to say anything against them. Latin was almost a necessity to a man of culture, and Greek was of use; but why should nature, which spoke to us in so many ways, be a dead language to us? And therefore, if it came to this question—Whether we were to have classes on special subjects in elementary schools, classes for grammar, predicates, and a great many long words which he hoped nobody would examine him in, or for science—he certainly should go in favour of science." These are weighty words coming from a man of Mr. Forster's experience and "common sense," and indeed make us hope that things are progressing, and that we shall not now have long to wait before science is introduced not only into colleges, but into schools of all grades. Mr. Forster concluded by admitting that the German workers were superior to ours in the fact that they added to practical training scientific knowledge, and that he saw no reason why in secondary and even university education voluntary efforts should not be seconded by State aid.

M. YVON VILLARCEAU has been appointed "Administrateur Provisoire" of the Paris Observatory by an order of the Minister for Public Instruction, dated Saturday last. M. Villarceau held a similar office after the death of Delaunay, before the reappointment of Leverrier. Nothing has been said yet as to the appointment of a successor.

AT the Guy's Hospital *conversazione*, on Monday evening, a new government filter, invented by Major Crease, was shown, which reduced strong tea and infusions of logwood to clear tasteless water. The nature of the filtering material is not made known.

THE white whale, which was brought from America and placed in a tank (50 feet by 25) of fresh water in Westminster

Aquarium last Wednesday week, unfortunately died on Saturday morning. In the course of the first few hours after being put in the water the whole of the skin, piece by piece, peeled off, and after this the whale appeared to be more comfortable, fed well, and adopted a less restless style of swim. The change in its condition in two days was remarkable. In consequence of its journey it had been for twelve days without food, and it was on arrival so thin that the spinous processes formed a ridge two or three inches high along the back. In two days, however, feeding only on eels, it had regained its normal appearance, but, as we say, expired on Saturday morning. A post mortem examination was made by Prof. Flower and Prof. Garrod, assisted by Dr. Bond, of Westminster Hospital and Mr. Henry Lee. Everything was in a healthy condition except the lungs, which had quite lost elasticity, and in which inflammation had evidently been set up some time. Plastic pneumonia was the cause of death. The stomach, notwithstanding the twelve days' fast, had been working naturally, and some partly-digested eels were found. There was abundance of healthy-looking flesh in all parts of the body, which was not expected. The skeleton is to be exhibited at the Aquarium, and the viscera and brain have been presented to the College of Surgeons. The specimen was a partly-grown female Beluga or White Whale, nine feet six inches long. Prof. Wyman, of Harvard, published a description of one he dissected in the *Boston Journal of Natural History*, vol. vii., giving it Lesson's name, *Beluga borealis*. In Europe it is generally called *Delphinapterus leucas* (Pallas), the generic name being that given by Lacépède in 1804. Some of the daily papers have unfairly commented on the whale being put in fresh water. If the writers had taken the trouble to turn to "White Whale" in Bell, they would have found this statement: "It is abundant in Hudson's Bay, Davis Straits, and the Arctic Ocean generally. . . . It seems partial to large rivers; in America it ascends the River St. Lawrence as far as Quebec, and in Asia Schrenk and Nordmann state that it goes far up the River Amur." It is stated that one was kept in fresh water in New York for three years, fed on eels. The weight of the brain of this Westminster specimen has been ascertained by Prof. Flower to be 63 oz., an unusually high amount in proportion to the size of the animal. One peculiarity of this whale is that all the cervical vertebræ are separate. Several details of practical importance with reference to the carriage of large cetaceans have been learnt from this experiment, among the most important of which is the fact that unless the water-tank, in which it seems most reasonable to suppose that they would best travel, is sufficiently large to allow of the tail being brought well into action the creature is certain to be drowned from inability to reach the surface that it may breathe. Considering the difficulties attending the enterprise it is surprising how the animal arrived in this country without a trace of injury; and that inflammation of the lungs should have been the cause of death in an aquatic species was equally little to have been expected.

PROF. QUINCKE, the successor of Kirchhoff at Heidelberg, is now in this country inspecting the various laboratories and collections of apparatus.

THE long-talked of Conference of Librarians commenced its meetings on Tuesday at the London Institution under the presidency of Mr. Winter Jones, librarian of the British Museum. Several papers were read on Tuesday and yesterday bearing on the organisation and utility of libraries, and we trust that the multitude who have for one purpose or another to deal with books and libraries will reap much benefit and decrease of worry by this congress of library officials. We would strongly commend to the attention of the members of the congress the letter published last week, from Prof. Mallet, of Virginia, advocating the organisation of a staff of searchers in connection with all our great libraries. Even the most devoted laboratory-worker must sometimes consult books, and it is desirable that this may be done with a minimum of waste of energy and time.

THE Birmingham and Midland Institute was opened for this session, on Monday, by Prof. Tyndall, who gave an interesting address which we are glad to see is printed at full length in the *Times*.

ADVICES have been received from the Howgate Arctic schooner *Florence*, dated Cape Breton, August 8, up to which time the vessel had had a very comfortable and satisfactory passage. The arrangements for the accommodation of the naturalist and meteorologist prove to be quite ample and satisfactory, and already collections of natural history of some interest have been made.

OUR Paris correspondent writes that important news has been received from M. de Brazza, the leader of the expedition to the Ogové, West Africa. Brazza writes from Doumé, a village beside one of the numerous cataracts of the river, in 0° 16′ S, and 13° 20′ E. The river is stated to flow from the south for a considerable distance, when it turns southward at or across the equator. The natives inform Brazza that the Ogové stretches a long way eastwards, and it is thought possible that it may come from some interior lake. Brazza seems to think that the Libumba, an affluent of the right bank of the Congo, may be also connected with the Ogové. As we hinted last week, it seems probable, since Stanley's discovery, that the Congo and Ogové are connected in some way.

THE sea-coast branch of the United States Fish Commission has been at work for some time. The steam tug *Speedwell*, a powerful vessel of 300 tons, commenced operations at Salem, Massachusetts, about August 1. Unexpectedly rich results were obtained in that vicinity, embracing not only many rare forms of animal life, but much of practical importance to the fisheries. Several places were found abounding in fish previously unknown to the fishermen of Gloucester and Marblehead. Flounders of marketable size in immense numbers were taken of a species (*Glyptocephalus cynoglossus*) previously entirely unknown on the American coast. Leaving Salem on August 19, it arrived at Halifax on Wednesday the 22nd, trawling and dredging the greater part of the way. In the course of this journey many new animals were collected of much interest to naturalists, among them several species of Greenland fish hitherto never detected south of that country.

IN Guido Cora's *Cosmos*, vol. iv. No. vi., we have an original chart of the Bay of Assab, accompanied by an elaborate description of the bay, the islands, and the adjacent continent, together with sailing directions. It appears to be somewhat better than the Red Sea Chart issued by the English Admiralty, but probably a drawing on a larger scale by Moresby or others is lying at the Hydrographical Office. The bay is on the African Coast, and is about forty miles from Perim Island, at the mouth of the Red Sea, and the same distance from Mocha. The most interesting point of this chart is that an area of some four miles by one mile and a-half is claimed for Italy.

M. HERPIN, an old professor of mathematics and cosmography, is about to publish, through Baudry of Paris, an astronomical dictionary, quite a novelty in French scientific literature, since the astronomical part of the great Encyclopædia was published at the end of the last century.

AN International Congress of Botany and Horticulture will be held at Paris during the International Exhibition next year. The Congress will open on August 16, 1878, and will last a week.

THE Cunard steamer *Abyssinia*, which arrived at Queenstown on Sunday, experienced fearful weather from the 22nd to 27th ult.—gales from west, north-west, to north. On the 25th, lat. 45·38 N., long. 41·56 W., she met a cyclone from north, and

was hove to for twenty-seven hours. This is believed to be a cyclone which recently started from the American coast and which thus vanished in the ocean.

THE geological survey of Brazil, which has been in progress for several years under the direction of Prof. C. F. Hartt, formerly of Vassar and Cornell Universities, United States, was lately for a short time threatened with suspension, but the proposal was countermanded and increased strength given to the commission after an investigation of all the circumstances. The temporary stoppage of operations was used advantageously by Prof. Hartt in placing the collections made by him in good order, and his parties have again entered the field in prosecution of their objects. Among the more important results so far accomplished by the survey has been the discovery of the existence in Brazil of the silurian, Devonian, carboniferous, triassic, Jurassic, cretaceous, and post-tertiary formations, all of them furnishing well-characterised fossils in great variety, and of which large numbers have been collected by the commission for its investigation, and for purposes of distribution in Brazil and of exchange with foreign establishments. So far no well-defined tertiary has been found to exist in Brazil. The survey has also been very successful in its ethnological researches, especially among the kitchen-middens of Santa Catharina, Paraná, Sao Paulo, Bahia, and the Amazonas, the results of which have been announced in part, although much of interest yet remains to be published. The researches in the coral reefs have been made the occasion of securing numbers of marine animals, all of which add to the resources of the survey. In connection with other operations, numerous photographs of scenery, of geological structure, and of the native races, have been taken.

THE death of Dr. Abraham Sager, an eminent anatomist and physiologist of the United States, took place on the 6th of August last. Dr. Sager, in 1837, was placed in charge of the botanical and zoological departments of the Michigan Geological Survey, and embraced this and subsequent opportunities to make large collections, which are now the property of the Michigan University. His investigations into the embryology and development of the tailed batrachians have added much to our knowledge of those forms.

THE bust of Sir Thomas Stamford Raffles, F.R.S., first president of the Zoological Society of London, has been placed in the new lion house of the Society's Gardens.

THE following foreign works have been sent us by Messrs. Williams and Norgate:—"Die kinetische Theorie der Gase," by Dr. Oskar Emil Meyer (Breslau); "Christian Gottfried Ehrenberg," by Johannes Hanstein (Bonn); "Phenomènes physiques de la Phonation," by J. Gadarret (Paris); "Ergebnisse physikalischer Forschung," by Dr. C. Bohn (Leipzig); "Physiologische Methodik," by Dr. Richard Gschiedlen (Braunschweig); "Synopsis Rubarum Germaniæ," by Dr. W. O. Focke (Bremen); "Lehrbuch der Analysis," by Rudolph Lipschitz: vol. I. (Bonn).

DR. F. A. FOREL, of Geneva, an energetic advocate of the doctrine of evolution, in an article published in the August number of the *Archives des Sciences physiques et naturelles*, proposes the application of natural selection for successfully healing certain diseases of silkworms, and also for rendering the European species of vines proof against the attacks of phylloxera. In the first matter experiments have already been made to a certain extent, and have been crowned with perfect success; in the case of vines the experiments are still to be made. The September number of the same journal, which is unusually bulky, is entirely devoted to a detailed biography of Auguste de la Rive, who died on November 23, 1873, at the age of seventy-two years.

WE have received a letter from Dr. Emil Bessels with reference to the *Polaris* observations (NATURE, vol. xvi. p. 358), and have much satisfaction in learning that it is proposed to revise the averages of the barometrical and thermometrical observations of the *Polaris* Arctic Expedition, these having been somewhat hastily prepared and published from a desire to have the report out before the expedition to the North Polar regions sailed from England.

A TELEGRAM from New York, September 30, states that the American Consul at St. John's (Newfoundland) has purchased from a seaman who was wrecked in Hudson's Bay, two spoons supposed to be relics of the Franklin Expedition, one of them being marked "J. G. F." It is said that Esquimaux living in the neighbourhood of Repulse Bay got them from a native chief, at whose camp the original owner, a white man, had died of scurvy. This statement does not seem quite consistent with the known facts as to the fate of the Franklin Expedition; moreover, we are not aware that "J. G. F." are Franklin's initials.

THE Museum of the Royal College of Surgeons of England has received as a present from the Hon. Charles P. F. Berkeley, the skeleton of a crocodile 15 feet 9 inches in length, which was shot by that gentleman last winter near Hagar Silsilis, in Egypt.

THE Swiss *Bundesrath* announces that the construction of the St. Gothard Tunnel is proceeding with increasing rapidity, and will probably be completed within three years.

THE seventh number (1877) of the *Bulletin* of the Belgian Academy of Sciences, contains a valuable paper, by M. C. Lagrange, "On the Influence of the Form of Bodies on their Attraction." This question, very incidentally treated by Brück, is thoroughly discussed by M. Lagrange, who arrives at some important conclusions. Discussing the attraction exercised by a body of irregular forms on a point situated at different distances from the centre of inertia of the body, and in different positions relatively to its axis of maximum and minimum inertia, the author proves that the attraction is directed to the centre of inertia only when the point is situated on one of the principal axes of inertia of the body; and that, at equal distances the attraction reaches its maximum when the point is on the axis of minimum inertia, and inversely, this maximum exceeding, and the minimum being less than, the attraction which would have been exercised were the whole mass of the body concentrated in its centre. Further, the author discusses the attraction exercised on a moving point, and arrives at the conclusion that the point, while attracted to the centre, will also receive an angular motion around the latter. Finally, he discusses the reciprocal attraction of two free bodies of irregular form, and, after having shown when the attraction will reach a maximum and a minimum, he proves also that the attraction will communicate to both bodies a rotatory motion, tending to bring into coincidence their axes of minimum inertia. In the two last paragraphs of his paper, M. Lagrange briefly notices the applications the principles he establishes may have in explaining the rotatory motion of the sun, as well as in accounting for crystallisation, further researches, not yet published, having enabled the author to account for the formation of different crystalline systems in a way which makes the whole question a problem of rational mechanics. The memoir is spoken of in very high terms by MM. Van der Mensbrugghe, Catalan, and De Tilly, who analysed it by order of the Academy.

NO. VIII. of the *Bulletin* of the United States National Museum consists of an "Index to the names which have been applied to the subdivisions of the class Brachiopoda," by Mr. W. H. Dall.

THE author of the work in the "Naturkräfte" series, on "Die Insecten," noticed in NATURE for September 13 (p. 418) is not Dr. Georg Mayr, but Prof. Vitus Graber, of Czernowitz University, the well-known author of numerous interesting monographs on insect anatomy and histology.

THE additions to the Zoological Society's Gardens during the past week include a Robben Island Snake (*Coronella phocarum*) from South Africa, presented by the Rev. G. H. R. Fisk; a Chimpanzee (*Troglodytes niger*) from West Africa, a Leonine Monkey (*Macacus leoninus*) from Arracan, a White-fronted Capuchin (*Cebus albifrons*), a Laughing Gull (*Larus atricilla*) from South America, deposited; a Greater Sulphur-crested Cockatoo (*Cacatua galerita*), from Australia, presented by Mr. G. S. S. Williams; two Red-backed Shrikes (*Lanius collurio*), European, presented by Capt. F. H. Sahrn; two Spotted Turtle Doves (*Turtur suratensis*), bred in the Gardens.

INTRODUCTION AND SUCCESSION OF VERTEBRATE LIFE IN AMERICA[1]

III.

THE artiodactyles, or even-toed ungulates, are the most abundant of the larger mammals now living, and the group dates back at least to the lowest eocene. In every vigorous primitive type which was destined to survive many geological changes, there seems to have been a tendency to throw off lateral branches which became highly specialised and soon died out because they are unable to adapt themselves to new conditions. The narrow path of the persistent suilline type throughout the whole tertiary is strewn with the remains of such ambitious offshoots, while the typical pig, with an obstinacy never lost, has held on in spite of catastrophes and evolution, and still lives in America to-day. The genus *Platygonus* is represented by several species, one of which was very abundant in the post-tertiary of North America, and is apparently the last example of a side branch, before the American suillines culminate in existing peccaries. The feet in this species are more specialised than in the living forms, and approach some of the peculiar features of the ruminants; as, for example, a strong tendency to coalesce in the metapodial bones. The genus *Platygonus* became extinct in the post-tertiary, and the later and existing species are all true peccaries. No authenticated remains of the genera *Sus*, *Porcus*, *Phacochœrus*, or the allied *Hippotamus*, the Old World suillines, have been found in America, although several announcements to that effect have been made.

In the series of generic forms between the lower eocene *Eohyus* and the existing *Dicotyles*, which I have very briefly discussed, we have apparently the ancestral line ending in the typical American suillines. Although the demonstration is not yet as complete as in the lineage of the horse, this is not owing to want of material, but rather to the fact that the actual changes which transformed the early tertiary pig into the modern peccary were comparatively slight, so far as they are indicated in the skeletons preserved, while the lateral branches were so numerous as to confuse the line. It is clear, however, that from the close of the cretaceous to the post-tertiary the bunodont artiodactyles were especially abundant on this Continent, and only recently have approached extinction.

The selenodont division of the artiodactyles is a more interesting group, and so far as we now know, makes its first appearance in the upper eocene of the west, although forms apparently transitional between it and the bunodonts occur in the dinoceras beds, or middle eocene. The most pronounced selenodont in the upper eocene is the *Oromeryx*, which genus appears to be allied to the existing deer family, or *Cervidæ*, and if so is the oldest known representative of the group. These facts are important, as it has been supposed, until very recently, that our eocene contained no even-hoofed mammals.

A most interesting line, that leading to the camels and llamas, separates from the primitive selenodont branch in the eocene, probably through the genus *Parameryx*. In the miocene, we find in *Pœbotherium* and some nearly allied forms unmistakable indications that the cameloid type of ruminant had already become partially specialised, although there is a complete series of incisor teeth, and the metapodial bones are distinct. In the pliocene the camel tribe was, next to the horses, the most abundant of the larger mammals. The line is continued through the genus *Procamelus*, and perhaps others, and in this formation the incisors first begin to diminish, and the metapodials to unite. In the post-tertiary we have a true *Auchenia*, represented by several species, and others in South America, where the alpacas and llamas still survive. From the eocene almost to the present time North America has been the home of vast numbers of the *Camelidæ*, and there can be little doubt that they originated here and migrated to the Old World.

The deer family has representatives in the upper miocene of Europe, which contains fossils strongly resembling the fauna of our lower pliocene, a fact always to be borne in mind in comparing the horizon of any group in the two continents. Several species of *Cervidæ*, belonging to the genus *Cosoryx*, are known from the lower pliocene of the west, and all have very small antlers, divided into a single pair of tynes.

The proboscidians, which are now separated from the typical ungulates as a distinct order, make their first appearance in North America in the lower pliocene, where several species of mastodon have been found. This genus occurs also in the upper pliocene and in the post-tertiary, although some of the remains attributed to the latter are undoubtedly older. The pliocene species all have a band of enamel on the tusks, and some other peculiarities observed in the oldest mastodons of Europe, which are from essentially the same horizon. Two species of this genus have been found in South America, in connection with the remains of extinct llamas and horses. The genus *Elephas* is a later form, and has not yet been identified in this country below the upper pliocene, where one gigantic species was abundant. In the post-pliocene remains of this genus are numerous. The hairy mammoth of the Old World (*Elephas primigenius*) was once abundant in Alaska, and great numbers of its bones are now preserved in the frozen cliffs of that region. This species does not appear to have extended east of the Rocky Mountains, or South of Columbia River, but was replaced there by the American elephant, which preferred a milder climate. Remains of the latter have been met with in Canada, throughout the United States, and in Mexico. The last of the American mastodons and elephants became extinct in the post-tertiary.

Perhaps the most remarkable mammals yet found in America are the *Tillodontia*, which are comparatively abundant in the lower and middle eocene. These animals seem to combine the characters of several different groups, viz., the carnivores, ungulates, and rodents. In the genus *Tillotherium*, the type of the order, and of the family *Tillotheridæ*, the skull resembles that of the bears; the molar teeth are of the ungulate type, while the large incisors are very similar to those of rodents. The skeleton resembles that of the carnivores.

We now come to the highest group of mammals, the primates, which includes the lemurs, the apes, and man. This order has a great antiquity, and even at the base of the eocene we find it represented by several genera belonging to the lower forms of the group. In considering these interesting fossils it is important to have in mind that the lemurs, which are usually regarded as primates, although at the bottom of the scale, are only found at the present day in Madagascar and the adjacent regions of the globe. All the American monkeys, moreover, belong to one group, much above the lemurs, while the Old World apes are higher still, and most nearly approach man.

In the lower eocene of New Mexico we find a few representatives of the earliest known primates, and among them are the genera *Lemuravus* and *Limnotherium*, each the type of a distinct family. These genera became very abundant in the middle eocene of the West, and with them are found many others, all, however, included in the two families *Lemuravidæ* and *Limnotheridæ*.

In the miocene lake basins of the West only a single species of the *Primates* has been identified with certainty. This was found in the oredoon beds of Nebraska and belongs to the genus *Laopithecus*, apparently related both to *Limnotheridæ* and to some existing South American monkeys. In the pliocene and post-pliocene of North America no remains of primates have yet been found.

In the post-pliocene deposits of the Brazilian caves remains of monkeys are numerous, and mainly belong to extinct species of *Callithrix*, *Cebus*, and *Jacchus*, all living South American genera. Only one extinct genus, *Protopithecus*, which em-

[1] Abstract of a lecture delivered at the Nashville meeting of the American Association, August 30, by Prof. O. C. Marsh. Continued from p. 472

braced animals of large size, has been found in this particular fauna.

It is a noteworthy fact that no traces of any anthropoid apes, or indeed of any Old World monkeys, have yet been detected in America. Man, however, the highest of the primates, has left his bones and his works from the Arctic Circle to Patagonia. Most of these specimens are clearly post-tertiary, although there is considerable evidence pointing to the existence of man in our pliocene. All the remains yet discovered belong to the well-marked genus *Homo*, and apparently to a single species, at present represented by the American Indian.

In this rapid review of mammalian life in America, from its first known appearance in the trias down to the present time, I have endeavoured to state briefly the introduction and succession of the principal forms in each natural group. If time permitted, I might attempt the more difficult task of trying to indicate what relations these various groups may possibly bear to each other; what connection the ancient mammals of this continent have with the corresponding forms of the Old World; and, most important of all, what real progress mammalian life has here made since the beginning of the eocene. As it is, I can only say in summing up, that the marsupials are clearly the remnants of a very ancient fauna, which occupied this continent millions of years ago, and from which the other mammals were doubtless all derived, although the direct evidence of the transformation is wanting.

The relations of the American primates, extinct and recent, to those of the other hemisphere, offer an inviting topic, but it is not in my present province to discuss them in their most suggestive phases. As we have here the oldest and most generalised members of the group, so far as now known, we may justly claim America for the birth-place of the order. That the development did not continue here until it culminated in man, was due to causes which at present we can only surmise, although the genealogy of other surviving groups gives some data towards a solution. Why the Old World apes, when differentiated, did not come to the land of their earlier ancestry, is readily explained by the then intervening oceans, which likewise were a barrier to the return of the horse and rhinoceros.

Man, however, came; doubtless first across Behring's Straits; and at his advent became part of our fauna, as a mammal and primate. In these relations alone it is my purpose here to treat him. The evidence, as it stands to-day, although not conclusive, seems to place the first appearance of man in this country in the pliocene, and the best proof of this has been found on the Pacific coast. During several visits to that region many facts were brought to my knowledge which render this more than probable. Man at this time was a savage, and was doubtless forced by the great volcanic outbreaks to continue his migration. This was at first to the south, since mountain chains were barriers on the east. As the native horses of America were now all extinct, and as the early man did not bring the Old World animal with him, his migrations were slow. I believe, moreover, that his slow progress towards civilisation was in no small degree due to this same cause, the absence of the horse.

It is far from my intention to add to the many theories extant in regard to the early civilisations in this country, and their connections with the primitive inhabitants or the later Indians, but two or three facts have lately come to my knowledge which I think worth mentioning in this connection. On the Columbia River, I have found evidence of the former existence of inhabitants much superior to the Indians at present there, and of which no tradition remains. Among many stone carvings which I saw there were a number of heads which so strongly resemble those of apes that the likeness at once suggests itself. Whence came these sculptures, and by whom were they made? Another fact that has interested me very much is the strong resemblance between the skulls of the typical mound-builders of the Mississippi Valley and those of the Pueblo Indians. I had long been familiar with the former, and when I recently saw the latter it required the positive assurance of a friend who had himself collected them in New Mexico to convince me that they were not from the mounds. A third fact, and I leave man to the archæologists, on whose province I am even now trenching. In a large collection of mound-builders' pottery, over a thousand specimens, which I have recently examined with some care, I found many pieces of elaborate workmanship so nearly like the ancient water-jars from Peru that no one could fairly doubt that some intercourse had taken place between the widely-separated people that made them.

The oldest known remains of man on this continent differ in no important characters from the bones of the typical Indian, although in some minor details they indicate a much more primitive race. These early remains, some of which are true fossils, resemble much more closely the corresponding parts of the highest Old World apes, than do the latter our tertiary primates, or even the recent American monkeys. Various living and fossil forms of Old World primates fill up essentially the latter gap. The lesser gap between the primitive man of America and the anthropoid apes is partially closed by still lower forms of men, and doubtless also by higher apes, now extinct. Analogy, and many facts as well, indicate that this gap was smaller in the past. It certainly is becoming wider now with every generation, for the lowest races of men will soon become extinct, like the Tasmanians, and the highest apes cannot long survive. Hence the intermediate forms of the past, if any there were, become of still greater importance. For such missing links, we must look to the caves and later tertiary of Africa, which I regard as now the most promising field for exploration in the Old World. America, even in the tropics, can promise no such inducements to ambitious explorers. We have, however, an equally important field, if less attractive, in the cretaceous mammals, which must have left their remains somewhere on this continent. In these two directions, as I believe, lie the most important future discoveries in palæontology.

As a cause for many changes of structure in mammals during the tertiary and post-tertiary, I regard as the most potent, *natural selection*, in the broad sense in which that term is now used by American evolutionists. Under this head, I include not merely a Malthusian struggle for life among the animals themselves, but the equally important contest with the elements, and all surrounding nature. By changes in the environment, migrations are enforced, slowly in some cases, rapidly in others, and with change of locality must come adaptation to new conditions, or extinction. The life history of tertiary mammals illustrates this principle at every stage, and no other explanation meets the facts.

The real progress of mammalian life in America, from the beginning of the tertiary to the present, is well illustrated by the brain-growth, in which we have the key to many other changes. The earliest known tertiary mammals all had very small brains, and in some forms this organ was proportionally less than in certain reptiles. There was a gradual increase in the size of the brain during this period, and it is interesting to find that this growth was mainly confined to the cerebral hemispheres, or higher portion of the brain. In most groups of mammals the brain has gradually become more convoluted, and thus increased in quality as well as quantity. In some, also, the cerebellum and olfactory lobes, the lower parts of the brain, have even diminished in size. In the long struggle for existence during tertiary time the big brains won, then as now; and the increasing power thus gained rendered useless many structures inherited from primitive ancestors, but no longer adapted to new conditions.

Another of the interesting changes in mammals during tertiary time was in the teeth, which were gradually modified with other parts of the structure. The primitive form of tooth was clearly a cone, and all others are derived from this. All classes of vertebrates below mammals, namely, fishes, amphibians, reptiles, and birds, have conical teeth, if any, or some simple modification of this form. The edentates and cetaceans with teeth retain this type, except the zeuglodonts, which approach the dentition of aquatic carnivores. In the higher mammals the incisors and canines retain the conical shape, and the premolars have only in part been transformed. The latter gradually change to the more complicated molar pattern, and hence are not reduced molars, but transition forms from the cone to more complex types. Most of the early tertiary mammals had forty-four teeth, and in the oldest forms the premolars were all unlike the molars, while the crowns were short, covered with enamel, and without cement. Each stage of progress in the differentiation of the animal was, as a rule, marked by a change in the teeth, one of the most common being the transfer, in form at least, of a premolar to the molar series, and a gradual lengthening of the crown. Hence it is often easy to decide from a fragment of a jaw, to what horizon of the tertiary it belongs. The fossil horses of this period, for example, gained a grinding tooth for each toe they lost, one in each epoch. In the single-toed existing horses all the premolars are like the molars, and the process is at an end. Other dental transformations are of equal interest, but this illustration must suffice.

The changes in the limbs and feet of mammals during the same period were quite as marked. The foot of the primitive

mammal was doubtless plantigrade, and certainly five-toed. Many of the early tertiary forms show this feature, which is still seen in some existing forms. This generalised foot became modified by a gradual loss of the outer toes and increase in size of the central ones, the reduction proceeding according to systematic methods, differing in each group. Corresponding changes took place in the limb bones. One result was a great increase in speed, as the power was applied so as to act only in the plane of motion. The best effect of this specialisation is seen to-day in the horse and antelope, each representing a distinct group of ungulates with five-toed ancestors.

If the history of American mammals, as I have briefly sketched it, seems as a whole incomplete and unsatisfactory, we must remember that the genealogical tree of this class has its trunk and larger limbs concealed beneath the *débris* of mesozoic time, while its roots doubtless strike so deeply into the palæozoic that for the present they are lost. A decade or two hence we shall probably know something of the mammalian fauna of the cretaceous, and the earlier lineage of our existing mammals can then be traced with more certainty.

The results I have presented to you are mainly derived from personal observation, and since a large part of the higher vertebrate remains found in this country have passed through my hands, I am willing to assume full responsibility for my presentation of the subject.

For our present knowledge of the extinct mammals, birds, and reptiles of North America, science is especially indebted to Leidy, whose careful, conscientious work has laid a secure foundation for our vertebrate palæontology. The energy of Cope has brought to notice many strange forms, and greatly enlarged our literature. Agassiz, Owen, Wyman, Baird, Hitchcock, Deane, Emmons, Lea, Allen, Gibbes, Jefferson, DeKay, and Harlan deserve honourable mention in the history of this branch of science. The South American extinct vertebrates have been described by Lund, Owen, Burmeister, Gervais, Huxley, Flower, Desmarest, Aymard, Pictet, and Nodot. Darwin and Wallace have likewise contributed valuable information on this subject, as they have on nearly all forms of life.

In this long history of ancient life I have said nothing of what life itself really is. And for the best of reasons, because I know nothing. Here at present our ignorance is dense, and yet we need not despair. Light, heat, electricity and magnetism, chemical affinity, and motion are now considered different forms of the same force; and the opinion is rapidly gaining ground that life, or vital force, is only another phase of the same power. Possibly the great mystery of life may thus be solved, but whether it be or not, a true faith in science admits no limit to its search for truth.

THE GERMAN ASSOCIATION AT MUNICH

THE fiftieth meeting of the German Association of Naturalists and Physicians began on September 17 by a large assembly of visitors in the old Town Hall at Munich. The meeting this year assumed quite a national character. Although in the programme its scientific character was principally considered, and pleasure trips, banquets, &c., had been reduced to the most modest proportions in comparison with former years, yet the aspect of the city of Munich, and of all the edifices that were in any way connected with the meeting, was a festive one. Some 2,000 visitors had arrived, and the Town Hall on the night of the 17th was crowded to suffocation. The authorities of the city gave a grand Keller-Fest in honour of the visitors on the 20th, which was attended by over 5,000 guests.

The first general meeting was opened by Prof. von Pettenkofer on the morning of the 18th inst. In a short address the professor announced that His Majesty the King had intended to send his royal greeting to the assembled men of science through H.R.H. Duke Carl Theodor, of Bavaria (brother to the Empress of Austria), but that the duke had suddenly been called to Dresden through the death of the dowager Queen of Saxony. In his absence His Majesty had intrusted the secretaries with this honourable message. After other congratulations Dr. von Pettenkofer delivered his inaugural address. He reminded the assembly that the present was a jubilee meeting, and then gave a retrospect of the growth of the Association since its foundation. The first meeting took place at Leipzig on September 18, 1822, when, following the invitation of Prof. Oken, twenty scientific men assembled and founded the Society. A paragraph of the statutes prescribed that the meetings should always begin on September 18, and should last several days. Under the political circumstances of that time and with the means of conveyance then existing the modest number of twenty members was considered a fair beginning. The next meeting occurred at Halle with thirty-four members, the third at Würzburg with thirty-six, the fourth at Frankfort-on-Maine with 110, the fifth at Dresden with 116, the sixth at Munich with 156, and the seventh at Berlin in 1828, when 464 members were present. The Association steadily increased and the meetings were held annually unless prevented by war or epidemics. The last meeting at Hamburg numbered over 2,000 members. Little by little a division of labour took place, and out of the seven original sections twenty-five have now resulted.

After speaking of the progress made by man as compared with the lower animals, Prof. Pettenkofer said—If knowledge is power, and nobody will doubt this, then amongst sciences natural science is certainly destined to play a great part, perhaps the greatest, in the history and culture of mankind. . . . Natural science has but to look for facts and truths, and need never busy itself about the immediate practical application of what has been found, because for them alone it deserves the sympathy of the entire civilised world, and the means necessary for its culture and development. No investment of capital bears higher interest. Finally, the speaker recalled the memory of Prof. Ludwig Lorenz Oken, the founder of the association and the author of the statutes which, with but a single and trifling exception are still in force to-day. He praised the patriotism of Oken, and regretted that he died before the reestablishment of the United German Empire.

At the end of the address, the assembly, at the request of Dr. von Pettenkofer, rose from their seats in honour of the memory of Oken.

Then followed the first scientific lecture, which was delivered by Prof. Waldeyer (Strassburg). He spoke on *Karl Ernst von Baer and his Influence on Natural Science*, giving an elaborate memoir of the late great naturalist, to whom we owe many of the bases of the present theory of evolution. Prof. Dr. Haeckel then delivered his address *On the Evolution Theory at the Present Time*, which we give elsewhere.

At the second general meeting, on the 20th, the choice of a place of meeting was made for next year, Cassel being selected, with Doctors Stilling and Gernau as secretaries. Duke Carl Theodor of Bavaria, himself an able ophthalmologist, took the chair in lieu of Dr. Pettenkofer, and again welcomed the assembly, in the name of the kingdom of Bavaria, in an interesting speech. Then followed the address of the eminent botanist, Prof. Dr. Nägeli, of Munich, "On the Limits of Natural Knowledge." He pointed out that many naturalists, when asked about the limits of natural knowledge, and thinking a solution by principles insufficient, simply reply that faith begins where knowledge ends. Humanity faces the whole of nature, masters new domains constantly by dint of meditation; the empire of knowledge thus always increases in extent, and that of faith decreases as constantly. But this solution does not satisfy our interest. We would wish to know particularly whether the limits of natural knowledge can be determined at all, and how far we can penetrate into nature. The solution of this question is determined by three conditions:—(1) By the condition and capacity of the investigating Self; (2) by the condition and accessibility of nature; and (3) by the demands which we make from knowledge. With regard to the first point, the undoubted fact is decisive that our power of thinking, in whatever condition it may be, can but give us nature as we perceive her with our five senses, and even this again not in her full extent and completeness, but only as far as we perceive her in the present. We see and hear only what is in the present; now as the organs of our senses are more or less sensitive for the one or the other perception, Darwin's ingenious idea that in nature only so many phenomena attained full development as were useful to the individual bearer, is fully justified; on the other hand it is very probable that many molecular forces exist of which we have no idea, simply because we cannot perceive them with our senses. The limited capacity of the I allows us only an extremely fragmentary knowledge of the universe. With reference to the second point, the condition and accessibility of nature, we can easily perceive the limit; for man it rests in space and time. The infinity of space and time, and its consequences, are insuperable for us, and nature is everywhere uninvestigable where she becomes eternal or infinite, and therefore she can never be *entirely* investigated. The naturalist must therefore always bear in mind that all his investigations are restricted to natural limits, otherwise he will lose himself in ground-

less fancies, and will arrive at absurd conclusions. The speaker then turned to the conceptions of the universe. The world which is known to us changes; if we follow this in the past and future we find, from a physical point of view, a state which approaches perfect rest more and more, without reaching it altogether. But if we suppose that in space worlds arise from worlds without end and perish again, then the successive states, according to the materialistic conception, are of the same value, while according to the philosophical conception they change their relative value by becoming more perfect. The one conception lets the world awaken from dead repose and return to it, the other condemns it to eternal repose. With regard to the extension of the universe in space, the thought that all material space must again and again have limits, leads us to the mathematical conclusion that our earth, just as it is now, reoccurs in infinite numbers in the universe. The speaker then passed to the third point, viz., the demands we make of knowledge. Our knowledge does not go further than to compare observed phenomena and judge of them with regard to others; we understand a phenomenon, understand its value with regard to other phenomena, if either we measure, count, or weigh it, or if we create it ourselves. It is in this latter manner that mathematical science is the product of our mind. The understanding of nature therefore rests in the recognition of the mathematical method in natural phenomena. As by the help of mathematics we understand only relative or quantitative differences, but not qualitative ones, because these cannot be compared, it follows that with regard to the latter scientific understanding is only possible separately within each single individual. Then Prof. Nägeli spoke against the opinion of those who divide nature into a material and a spiritual one, because no naturalist can avoid the conception of a causal connection of mind and body. The finite human mind is a double one; on the one hand it invents and puts the muscles into motion, on the other it contemplates, feels pleasure and pain, hate and love, and remembers. Even without this latter property, therefore, without consciousness, the world would have become world, man would have lived and taught, spoken and made music, but everything only mechanically—man would have been an automaton. Prof. Nägeli then passed from the domain of the mind to that of sensation, explaining that doubtless there was sensation in *all* molecular forces, the *same* sensations in the highest as well as in the lowest stages of organs, in the former only so much more vivid and refined than in the latter. If we understand spiritual life to be the mediator of cause and effect, then we find it everywhere. Du Bois-Reymond, who treated the same subject at Leipzig in 1872, finished his address with the words "Ignoramus et ignorabimus," but Prof. Nägeli ended *his* speech with the proud words—"We know, and we shall know if we are satisfied with human insight."

An address by Prof. Dr. Klebs, of Prague, followed "On the Changes in Medical Views during the Last Decades."

At the final meeting, on the 22nd instant, Prof. Rudolf Virchow gave an address "On the Liberty of Science in Modern State-life," which was received with loud acclamations of approval. After contrasting former with present times, Prof. Virchow said that the last few days had proved that now science enjoys full liberty. We must retain this possession, and must take care not to go too far. Moderation, the resignation of personal predilections, will be necessary to retain the present favourable conditions. The sum total of that which we may designate as true and real science, in the strictest sense of the word, and for which alone we may demand full scientific liberty, is a far more modest one than the domain of speculative expansion of problems and of presentiment. The speaker then in the most detailed and interesting manner drew the limit between speculative investigation on the one side and that which we have recognised as facts on the other. Prof. Virchow is ready to ask that everything which may be considered as a perfectly secured scientific truth, shall be admitted to the scientific treasure of the nation. If now we stand everywhere before reforms in education, and if for natural science a far-reaching consideration is claimed, it must first of all be perfectly clear to us *what* is to be comprised in this science and *what not*, and it cannot be left for the pedagogues to decide, as Prof Haeckel says it ought to be, whether the doctrine of evolution is to be comprised in the programme of education or not. If this doctrine is a scientific truth, and proved beyond doubt, then its admission to this educational programme *must* be demanded, unless we wish to make hypocrites of our teachers. But if it is completely proved it ought to be explained to every child in the schools, not only to the scientific man. The speaker then criticised somewhat severely Prof. Haeckel's theory of the plastidule soul and of the animated cell. As long as the undeniable proofs were wanting, he maintained, we ought, on the contrary, to ask our teachers *not* to teach the evolution doctrine. In the domain of the doctrine of evolution wise moderation is more necessary than anywhere else. For many years Harvey's maxim, "Omne vivum ex ovo" remained undenied; to-day we know for certain that the "omne" is incorrect. In the same way the "generatio æquivoca" may be true or not it certainly is not undeniably proved. In natural science belief and knowledge, *i.e.*, subjective and objective knowledge are united. The domain of dogmatic belief is lessened year after year in favour of objective knowledge which is based upon facts. But apart from the latter, subjective knowledge makes itself very prominent sometimes, and hallucinations and fancies are now and then hid beneath its cover. Anthropological investigations contradict directly the doctrine of evolution. The skulls found in the tombs of the oldest times show a far more human and a far less apish type than do a great many living heads, and we cannot suppose that only the highest-developed skulls of those periods have escaped destruction. Therefore, precaution, moderation, no overrating of our scientific power, for Bacon's "scientia est potentia" is only meant for true objective knowledge.

Many papers of great scientific value were read in the various sections, and we hope to be able to refer to these in a future number.

THE PRESENT POSITION OF THE EVOLUTION THEORY[1]

On this festive day which unites us here for the opening of the fiftieth meeting of the Association of German Naturalists, universal science may justly point out its relation to the domains of our special investigations. On such a day the educated of all circles, who follow with vivid interest the astonishing progress of the investigation of nature are specially to ask what general results have been obtained for the entire domain of human education. If, therefore, to-day I comply with the honourable request addressed to me, and ask for your kind attention for a short time, I do not think that I can choose a more fitting subject for our common consideration than the relation of science as a whole to that branch of investigation which lies nearest to me, viz., the doctrine of evolution.

No other doctrine has so vividly claimed general attention for the last decade, no other affects our most important convictions so deeply, than the newly-risen doctrine of evolution and the monistic philosophy united with it. Because wholly and solely by this doctrine the "question of all questions" can be solved, the fundamental "question of the position of man in nature." As man himself is the measure of all things, thus naturally the last fundamental questions and the highest principles of all science must depend on the position which our advanced understanding of nature assigns in nature to man himself.

As you know, it is principally to Charles Darwin that the evolution theory of the present day owes this commanding position. Because it was he who, eighteen years ago, first broke through the rigid ice-cover of reigning prejudices, inspired by the same fundamental thought of a monistic development of the world, which a century ago moved our greatest thinkers and poets, Immanuel Kant and Wolfgang Goethe at their head. By the conception of his theory of selection—the doctrine of natural selection in the struggle for existence—Darwin could in particular give a firm foundation to the most important biological part of that doctrine, which had already appeared in the beginning of our century, viz., the theory of descent. In vain the older natural philosophy had then begun the fight for this theory; neither Lamarck and Geoffroy St. Hilaire in France, nor Oken and Schelling in Germany could obtain a victory for it. Just fifty years have now passed since Lorenz Oken began his academical lectures on the theory of evolution here at Munich, and it therefore becomes us here to-day to place a laurel wreath upon the tomb of this deep-sighted zoologist and inspired philosopher. It was Oken also who, in his enthusiasm for scientific unity, called together the first meeting of German naturalists at Jena in 1822, and to whom, for that reason alone, the thanks of this fiftieth assembly are due.

But the natural philosophy of that time could only draw up the general plan of construction and the first outline of the colossal edifice of the monistic theory of evolution; only the zealous and ant-like diligence of half the following century collected the

[1] "On the Evolution Theory of the Present Day in its Relation to Science in general." Address by Prof. Haeckel at the Munich Meeeting of the German Association.

building material for its execution. An immense literature and an admirable perfection of the methods of investigation now give the most brilliant proof of the astonishing progress of the empirical science of nature during that period. But of course the immeasurable widening of the field of empirical observation, and the special division of labour caused by this, often led to an unfortunate dispersion of powers; the higher object of the recognition of general laws was often entirely forgotten in the nearer interest in the observation of details.

Thus it could happen that while this strictly empirical investigation of nature was flourishing at its highest in the years 1830 to 1859, during thirty years, the two principal branches of real natural *history* started from totally different bases. In the history of the development of the earth the conviction gained ground more and more since 1830, the year of the publication of "Lyell's Principles of Geology," that our planet had neither been formed by a supernatural act of creation, nor had passed through a series of total revolutions of mystical origin, but that, on the contrary, a gradual and uninterrupted development had caused its natural formation step by step. On the other hand, in the history of development of the living inhabitants of the earth the old irrational myth remained in full force, according to which every single species of animals and plants, like man himself, had been created independently of one another, and that a series of such creations had followed each other without any genetic connection. The glaring contradiction of the two doctrines, of the natural development-theory of geologists, and of the supernatural creation myth of biologists, was only decided in favour of the former by Darwin in 1859. Since then we recognise clearly that the formation and change of forms of the living inhabitants of our globe follow the same great eternal laws of mechanical development as the earth itself and the whole world-system.

We need not to-day, as we were obliged to do fourteen years ago at the meeting of naturalists at Stettin, cite the reasons and proofs for Darwin's new theory of development. The recognition of its truth has since made its way in the most satisfactory manner. In that domain of natural investigation to which my own labours belong, in the wide field of the science of organic forms or *morphology*, it is already recognised everywhere as the most important basis. Comparative anatomy and the history of germs, systematic zoology and botany cannot to-day do without the theory of descent. Because only by its light the mysterious relations of the numberless organic forms amongst each other can be really explained, *i.e.*, reduced to mechanical causes. Their similarity results as the natural consequence of *inheritance* from common parental forms, their variation as the necessary effect of *adaptation* to different conditions of life. Only by the theory of descent can the facts of palæontology, of chorology, and of œcology, be explained in a way as simple as it is natural; only by this theory we understand the existence of the remarkable rudimentary organs, of the eyes which do not see, the wings which do not fly, the muscles which do not move—nothing but useless parts of the body, which refute in the most emphatic manner the old-fashioned *teleology;* because they prove in the clearest manner that the utility in the structure of organic forms is neither general nor perfect; that it is not the result of a plan of creation worked with an object in view, but necessarily caused by the accidental coincidence of mechanical causes.

Who, in the face of these overwhelming facts, still asks today for proofs of the theory of descent, proves by that only his own want of knowledge or reason. But it is utterly wrong to demand exact or indeed experimental proofs. This demand, which is so often heard, results from the widely-spread error that all natural science must be exact; all the other sciences are often confronted with this, under the name of "spiritual or pure sciences" (*Geisteswissenschaften*). Now in truth, only the smaller part of natural science is exact, viz., that part which can be proved *mathematically;* astronomy before all others, and higher mechanics in general; after these the greatest part of what remains of physics and chemistry, also a good part of physiology, but only a very small part of morphology. In this latter biological domain the phenomena are far too complicated and variable to allow of our applying the mathematical method at all. If indeed the demand for a foundation, which shall be as exact as possible, and mathematical if possible, stands good in principle for *all* sciences, it is yet quite impossible to carry this through in by far the greater part of the biological disciplines. Here, on the contrary, the historical and historico-philosophical method takes the place of the exact, mathematical, and physical one.

This applies to morphology before all others, because the scientific understanding of organic forms we obtain solely through the *history of their development.* The great progress of our time in this domain consists in our conceiving the meaning and object of the history of development in an infinitely wider sense than has been done before Darwin. Up to his time it meant only the history of the formation of the organic individual form, which to-day we call *history of the germ*, or *ontogeny*.

If the botanist followed the formation of the plant from the seed, the zoologist that of the animal from the ovum, they considered their morphological task accomplished by the perfect observation of the history of these germs. The greatest men in the domain of the history of evolution, Wolff, Baer, Remack, Schleiden, and the whole school of embryologists formed by them, understood by it, until a short time ago, the individual ontogeny exclusively. It is quite different to-day, when the mysteries of the wonderful history of germs confront us no longer as unintelligible riddles, but have clearly revealed their deep significance; because according to the laws of inheritance, the changes of form which the germ passes through in the shortest time, under our eyes, are a compressed and abbreviated repetition of the corresponding changes of form, which the ancestors of the organism in question have passed through in the course of many millions of years. If to-day we place a hen's egg into the breeding machine, and if twenty-one days later we see a little chicken creep from it, we no longer remain in mute astonishment at the wonderful changes which lead from the simple cell in the egg to the two-leaved gastrula, from this to the worm-shaped and skull-less germ and thence to further germ-forms, which on the whole show the organisation of a fish, an amphibian, a reptile, and only lastly that of a bird. On the contrary, we draw conclusions from this regarding the corresponding series of forms of the ancestors, which have led from the unicellular amœba to the parental form of the gastræa, and further on through the classes of worms, acephala, fishes, amphibia, reptiles, down to birds. The series of germ-forms of the chicken thus gives us a sketch of the series of its real ancestors.

Our biogenetic fundamental law gives the immediate causal connection which thus exists between the ontogeny of any organic individual form and the history of the forms of its ancestors in the following short phrase:—*The history of the germ is an extract from the history of its ancestors*, occasioned by the laws of inheritance. This *palingenetic* extract appears essentially disturbed only in case, through adaptation to the conditions of embryonal life, *cenogenetic* changes have taken place.

This phylogenetic interpretation of the ontogenetic phenomena is, up to the present, the only explanation of the latter. But it receives the most important confirmation and supplementation from the results of comparative anatomy and palæontology. It is of course impossible to prove this by an exact method or indeed an experiment, because all these biological disciplines are, according to the nature of the matter, *historical* and philosophical natural sciences. Their common object is the investigation of historical events, which happened in the course of many millions of years, long before the appearance of the human race on the surface of our youthful planet. The immediate and mathematically exact conception of these events is therefore altogether beyond the reach of possibility.

Only by the critical consideration of the *historical archives*, by a speculation which is just as circumspect as it is daring, an approximate understanding here becomes indirectly possible. Phylogeny uses these historical archives in the same manner and according to the same method as other historical disciplines do. Just as the historian, by the help of chronicles, biographies, and letters draws up a detailed representation of an event long past; as the archæologist by the study of inscriptions, pieces of sculpture, utensils, obtains the knowledge of the state of civilisation of a race long extinct; as the linguist by comparative investigation of all related living languages and their older written documents proves their development and origin from a common ancestral language; just in the same manner the naturalist of to-day, by the critical use of the phylogenetic archives, of comparative anatomy, ontogeny, and palæontology, arrives at an approximate understanding of the events which, in the course of unmeasured periods, have caused the change of forms in the organic life upon our globe.

The history of the parental forms of organisms, or *phylogeny*, can therefore be proved by an exact method or by experiment just as little as this is the case with her older and more favoured sister *geology*. But the high scientific value of the latter is never-

theless now generally acknowledged. Only the ignorant to-day smile incredulously at the explanation that the colossal mountain chains of the Alps, the snow-covered summits of which we see glistening in the far distance, are nothing else but the hardened deposits of the sea. The structure of these stratified mountains and the nature of the fossils they inclose do not admit of another explanation; and yet it cannot be proved in an exact way. In the same manner all geologists now unanimously suppose a certain systematic succession of the mountain strata, corresponding to their different ages; and yet this system of strata is nowhere perfectly present upon the earth. But our phylogenetic hypotheses may claim the same value as is given to these generally recognised geological hypotheses. The only difference is that the enormous structure of hypotheses in geology is far more perfect, simple, and easier to understand, than that of youthful phylogeny.

Thus these *historical sciences of nature*, geology and phylogeny, now form the uniting bond between the exact natural sciences on the one hand, and the historical sciences of the intellect, or pure sciences, on the other. The whole of biology, in particular systematic zoology and botany, are thus raised to the rank of a true natural *history*, an honourable title, which these sciences have borne long ago, but which they only now merit truly. If indeed to-day in many quarters, even in official ones, they are designated as "descriptive natural sciences," and opposed to the "explanatory" ones, this only shows what a false idea had hitherto been entertained of their true object. Since the "natural system" of organisms has been recognised as their *ancestral pedigree*, the living phylogeny of classes and species takes the place of dead descriptive systematics.

However highly we may estimate this enormous progress of morphology, yet it would not suffice by itself to explain the extraordinary effect of the evolution doctrine of to-day upon science in general. This, as you know, rests upon a single special deduction drawn from the theory of descent, upon its application to *man*. The very old question of the origin of our own race is by this theory solved for the first time in a natural scientific sense. If the theory of evolution is true at all, if there exists a natural phylogeny at all, then man also, the crown of creation, has resulted from the form *vertebrata*, from the class *mammalia*, from the sub-class *placentalia*, from the order *apes*. If Linnæus, in 1735, in his system of nature, already united man with apes and bats in the order of primates, if all following zoologists could not move him out of the class of mammalia, then this unanimously recognised systematic position can, phylogenetically, only be interpreted as descent from that class of animals.

All attempts to shake this most important deduction from the evolution doctrine are futile; it is vain to try to keep a particular exceptional position for man, by constructing for him a special line of ancestors, separated from those of the vertebrata. The phylogenetic archives of comparative anatomy, ontogeny, and palæontology, speak too distinctly in favour of an identical and uniform (*einheitlich*) descent of all vertebrata from a single common ancestral form, to permit of our having any doubts on this subject now. Not a single investigator and comparer of languages thinks it possible that languages as widely different as the German, Russian, Latin, Greek, and Indian languages have developed from different original languages. On the contrary, all linguists, by critical comparison of the structure and the development of these different languages, arrive unanimously at the conviction that they all have emanated from a single Aryan or Indo-Germanic mother language. Just in the same way all morphologists arrive at the firm conviction that all vertebrata, from the *amphioxus* upwards to man himself, all fishes, amphibia, reptiles, birds, and mammals descend originally from a single vertebrate ancestor; for we cannot imagine that all the different and highly-complicated conditions of life, which, through a long series of processes or stages of development, led to the typical formation of a vertebrate, have accidentally happened together more than once in the course of the earth's history.

For our consideration to-day only the general conception of the vertebrate-origin of man is of importance, we will not occupy our time with the single ancestral stages of our pedigree. I would only in passing point out that at least the principal stages of the same are now considered as firmly established, thanks to the excellent labours of our most illustrious morphologists, Gegenbaur and Huxley before all others. Of course it is still often supposed that thus, even to-day, only the origin of the human body is explained, but not that of our spiritual activity. In the face of this important objection we must remember, before all else, the physiological fact, that our intellectual life is inseparably united with the organisation of our central nervous system. The latter, however, is composed exactly like that of all higher vertebrata, and originates in exactly the same way. Also, according to Huxley's investigations, the differences between the structure of the brain of man and that of the higher apes are far less important than the corresponding differences between the higher and lower apes. Now as the function or work of each organ cannot be imagined without the organ itself, and as the function is everywhere developed along with the organ, we are forced to suppose that our psychical activity has developed slowly and gradually in connection with the phylogenetic development of our brain.

For the rest this highly significant "soul question" appears to us in quite a different light to-day from what it did twenty, yes, even ten, years ago. Whatever we may imagine to be the nature of the connection of soul and body, of mind and matter, so much results with perfect clearness from the evolution doctrine of to-day that at least all organic matter—if indeed not all matter—is, in a certain sense, animated. First of all, we have been taught by advanced microscopical investigation, that the anatomical elementary parts of organisms, the *cells*, universally possess individual animated life (*allgemein ein individuelles Seelenleben besitzen*). Since Schleiden founded, forty years ago at Jena, the highly-significant cell theory for the vegetable kingdom, and Schwann soon afterwards applied the same to the animal world, we universally ascribe to these microscopical life-beings an individual and independent life; they are the true "individuals of the first order," the "elementary organisms" of Brücke. The grand and highly fertile application which Virchow, in his "Cellular Pathology," made of the cell theory with regard to the entire domain of theoretical medicine, is indeed based upon his considering the cells no longer as the dead passive building stones of the organism, but as the living, active state citizens of the same.

This conception is finally confirmed by the study of infusoria, amœbæ, and other unicellular organisms, because here we find with the single cells, living in isolation, the same manifestation of soul-life, sensation, and conception, volition and motion, as with the higher animals, composed of many cells! Both in the case of these latter social cells, as well as in that of the former hermit-cells, the soul-life of the cell is tied to one and the same most important cell substance—*protoplasm*. We even see in the monera and other most simple organisms that single detached pieces of protoplasm possess motion and sensation, just like the whole cell. Accordingly, we must suppose that the cell-soul, the foundation of empirical psychology, is a compound itself, namely, the total result of the psychic activities of the protoplasm-molecules, which we shortly call plastidule. The plastidule-soul would therefore be the last factor of organic soul-life.

But has the evolution doctrine of the present day thus exhausted its psychological analysis? Not at all! On the contrary, we are taught by modern organic chemistry that the peculiar physical and chemical properties of an element, of *carbon*, in its complicated combination with other elements, cause the peculiar physiological properties of organic compounds, and before all others, of protoplasm. The monera, consisting exclusively of protoplasm, here form the bridge over the deep chasm between organic and anorganic nature. They show us how the simplest and oldest organisms must have originally sprung from anorganic carbon compounds. If therefore in spontaneous generation a certain number of carbon atoms unite with a number of atoms of hydrogen, oxygen, nitrogen, and sulphur to form the unity of a plastidule (or molecule of protoplasm), we must regard the plastidule-soul, *i.e.*, the total sum of its life-activities, as the necessary product of the forces of these united atoms. The sum of the central atomic forces we may call atom-soul in a consequentially monistic sense. By accidental meeting and varied combination of the constant and unchangeable atom-souls the diverse and highly variable plastidule-souls originate, the molecular factors of organic life.

Arrived at this most extreme psychological consequence of our monistic doctrine of evolution, we meet with those old conceptions of the animation of all matter, which already in the philosophy of Democritus, Spinoza, Bruno, Leibnitz, and Schopenhauer have found varied expression; because all soul-life can finally be reduced to the two elementary functions of *sensation* and *motion*, to their reciprocal action in reflex motion. The simple

sensation of inclination and disinclination (*Lust und Unlust*), the simple form of motion, *attraction* and *repulsion*, these are the true elements out of which all soul-activity is built in infinitely varied and complicated combination. "The hating and loving of atoms," attraction and repulsion of molecules, motion and sensation of cells, and of the organisms composed of cells, the formation of thought, and the consciousness of man, these are only different stages of the universal psychological process of evolution.

The unity in the conception of the universe (or "monism") to which the new doctrine of evolution thus leads us, annuls the opposition which hitherto existed between the different dualistic world systems. It avoids the one-sidedness of materialism as well as that of spiritualism, it unites the practical idealism with the theoretical realism, it combines natural science with mental science (*Geisteswissenschaft*) to form an all-comprising uniform general or total science.

As thus we recognise the evolution doctrine of to-day to be a uniform and uniting cement of the most heterogeneous sciences, it gains the highest significance not only for the pure and theoretical but also for the practical and applied sciences. Neither practical medicine as an applied natural science, nor practical politics, jurisprudence and theology, in as far as they are parts of applied philosophy will in future be able to escape its influence. On the contrary we are convinced that it will prove, on all these domains, to be the most important lever of progressive knowledge as well as of ennobled civilisation in general. Now as the most important point of attack of the latter is the education of the young, the evolution doctrine will have to claim its just influence in the school as the most important means of education; here it will not be only tolerated, but it will become a ruling and guiding element.

If, finally, we are allowed to indicate, in a few words, at least the most important points of this relation, we may first of all lay stress upon the high significance of the genetic method in itself. Teachers as well as those they teach will contemplate each subject of their studies with infinitely greater interest and understanding, if, before all else, they ask themselves, "How did this originate? How did it develop itself?" Because in this question as to development the question as to the causes of facts is comprised; but after all it is always the recognition of the effecting causes, not the mere knowledge of facts, which satisfies the constant want of causalities of our mind. The recognition of common simple causes for the most varying and complicated phenomena leads to the simplification as well as to the deepening of our education and culture; only by causal conception dead knowledge becomes living science. Not the quantity of empirical knowledge, but the quality of its causal conception, is the true measure of the education of the mind.

How far the outlines of the general doctrine of evolution are now to be introduced into schools, in what succession its most important branches—cosmogony, geology, phylogeny of animals and plants, anthropogeny—are to be taught in the different classes, this we must leave to practical pedagogues to determine. But we believe that a far-reaching reform of education is unavoidable in this direction, and that it will be crowned with the most perfect success. How infinitely, for instance, the important teaching of languages will gain in educational value, if it is done comparatively and genetically! How the interest in physical geography will grow if it is genetically taught together with geology! How the tedious, dead systematics of the species of animals and plants will gain life and light if the two are explained as different branches of a common pedigree! And what a different conception we will, before all else, obtain of our own organism if we recognise it no longer as the fictitious likeness of an anthropomorphous creator, but in the clear daylight of phylogeny as the highest developed form of the animal kingdom; as an organism, which in the course of many millions of years has developed itself gradually from the line of vertebrate ancestors, and has far surpassed all its relatives in the struggle for existence!

As the doctrine of evolution will thus act in a fertilising and furthering way upon all branches of education, it will at the same time produce the consciousness of their monistic connection in the minds of both teachers and pupils. As historical natural science it will step as mediator and conciliator between the two opposed directions which to-day compete for power in the higher educational schools; on the one side the older, classical, historical, philosophical direction, on the other the newer, exact mathematical, physical direction. Both directions of education we think equally justified and equally indispensable; the human mind will only reach its full harmonious education, if both are equally taken into account. If formerly classical education was favoured too exclusively and one-sidedly, this has happened only too often recently with exact education. Both excesses the doctrine of evolution reduces to their proper measure, as it steps as a uniting bond between exact and classical science, between that of nature and that of the mind. Everywhere it teaches the living course of the connected, monistic, and uninterrupted development. Everywhere it shows to the zealous investigator new scientific aims beyond those already attained, and thus "gently leads the striving mind nearer and nearer to truth." The infinite perspective of progressive perfection which the doctrine of evolution thus opens before us is at the same time the best protest against the unfortunate "Ignorabimus," which it is obliged to hear now from many quarters, because nobody can predict what "limits of natural understanding" the human mind in the further course of its astonishing development will yet overstep in future!

By far the most important and most difficult demand which practical philosophy addresses to the evolution doctrine seems to be that of a new doctrine of morals (*Sittenlehre*). It is certain that afterwards, as before, the careful training of the moral character will remain the principal task of education. But up to the present the widest circles held the conviction firmly that this most important problem could only be solved in connection with certain ecclesiastical dogmas. Now as these dogmas, particularly in their union with very old myths of creation, directly contradict the principles of the doctrine of evolution, it was believed that through the latter religion and morals were endangered in the highest degree.

We consider this fear an erroneous one. It arises from the constant mixing up of the true and reasonable natural religion with the dogmatic, mythological church religion. The comparative history of religions, an important branch of anthropology, acquaints us with the great variety of external shells, in which the different people and times, according to their individual character and requirements, clothe religious thought. It shows us that the dogmatic teachings of church religions themselves are in a slow uninterrupted course of development. New churches and sects arise, old ones perish; at the best a certain form of creed lasts a few thousand years, an insignificantly small lapse of time in the æon-series of geological periods. Finally we are also taught by the comparative history of culture, how little true morality is necessarily united with a certain ecclesiastical creed. Often the greatest coarseness and decay of morals go hand in hand with the absolute power of an almighty church. We need only think of the middle ages! On the other hand we see the highest stage of moral perfection attained by men who have separated themselves from all ecclesiastical beliefs.

Independently of all church creeds, the germ of a true religion of nature lives in the breast of every man; it is connected inseparably with the noblest features of human existence itself. Its highest command is *love*, the restriction of our natural egotism in favour of our fellow men, for the benefit of human society, of which we are the members. This natural moral law is far older than all church religion. It has developed from the social instincts of animals. With animals of very different classes, particularly with mammals, birds and insects, we find its beginnings. According to the laws of association and of division of labour, many individuals here unite to form the higher community, called a state or hive. Its existence is necessarily connected with the reciprocal action of the members of the community, and with the sacrifices they make to the whole at the expense of their egotism. The consciousness of this necessity, *the feeling of duty*, is nothing else but a social instinct. But instinct is always a psychic habit, which, acquired originally by adaptation, has become inheritable in the course of generations, and finally appears as innate.

To convince ourselves of the admirable power of the animal feeling of duty, we need only destroy an ant-hill. There we at once see in the midst of destruction thousands of zealous state citizens occupied not with the salvation of their own dear lives, but with the protection of the cherished community to which they belong. Courageous warriors of the ant state set themselves up in powerful defence against our interfering finger; those that tend the young save the so-called "ants' eggs," the beloved pupæ, upon which rests the future of the state; diligent workers at once begin with undaunted courage to clear away the *débris*, and to construct new dwellings. The admirable organisation of these ants, of bees and other social animals, have originally developed from the crudest beginnings, just in the same manner as did our own human civilisation.

Even those most tender and most beautiful features of the human mind, which we principally glorify in poetry, we find already formed in the animal kingdom. Have the intense maternal love of the lioness, the touching matrimonial love of parrots ("inseparables"), the sacrificing faithfulness of the dog not been proverbial for ages? The most noble feelings of compassion and love, which determine actions, are here as with man, nothing but ennobled instincts. In connection with this conception, the ethics of the evolution doctrine need not look for new maxims, but reduce the very old commands of duty to their natural scientific base. Long before the origin of all church religion these natural commands of duty ruled the lawful living together of mankind as well as of social animals. Church religion ought to profit by this significant principle, not to combat it; for the future does not belong to that theology which conducts a fruitless battle against the victorious doctrine of evolution, but to that one which takes possession of it, recognises and uses it.

Therefore, far from fearing a shaking of all valid moral laws, and an obnoxious emancipation of egotism by the influence of the evolution doctrine upon our religious convictions, we, on the contrary, expect from it a reasonable confirmation of the moral doctrine on the unshakable basis of firm natural laws; for with the clear conception of our true position in nature, anthropogeny opens to us at the same time an insight into the necessity of our very old precepts of social duty. Henceforth practical philosophy and pedagogics will, like theoretical general science, deduce their most important maxims, not from supposed revelations, but from the natural principles of the doctrine of evolution. This victory of monism over dualism opens to us the most hopeful prospect for an infinite progress of our moral as well as of our intellectual development. In this sense we greet the evolution doctrine of to-day, as recently founded by Darwin, as the most important impulse of the whole of our pure and applied sciences.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—The Oxford University Commissioners having decided upon suspending two out of the three fellowships now vacant at All Souls' College, only one fellow will be elected in November.

BRISTOL.—The introductory lectures at the opening of University College, Bristol, commence on the 9th inst. Prof. Letts opens the chemistry class on the 10th with an address on "Old and New Views on the Nature of Matter," and Prof. S. P. Thompson the class of experimental physics on the 12th, with an address on "The Methods of Physical Science." The evening classes will be opened about a week later. Mr. J. F. Main, B.A., D.Sc., Scholar of Trinity College, Cambridge, has been appointed Lecturer in Mathematics and Applied Mechanics.

LEEDS.—The Yorkshire College, as it is now called, has published a neat calendar of about 100 pages in the orthodox grey colour characteristic of similar publications. The calendar contains all needful information on the organisation and business of the College, which now possesses six chairs, representing the main departments of science and literature, besides a chair of civil and mechanical engineering and one of textile industries. Judging from the course of study laid down for each class, and from the reputation of the professors, a high-class liberal education is now within easy reach of all Yorkshiremen. The calendar includes a prospectus of the Leeds' School of Medicine. For the coming session a much extended system of outside lecturing is announced, especially the arrangement made with the Gilchrist Trustees, through their secretary, Dr. W. B. Carpenter, F.R.S., by which some of the college professors will deliver four series of "Science Lectures for the People" in Leeds, Bradford, Halifax, and Keighley.

SOCIETIES AND ACADEMIES

LONDON

Entomological Society, September 5.—Prof. J. O. Westwood, M.A., president, in the chair.—Mr. F. Smith exhibited, on behalf of Mr. G. A. J. Rothney, a remarkably fine collection of Hymenoptera from Calcutta. Among them were several new species of *Cerceris* and a few new species of *Apidæ*.—Mr. McLachlan exhibited drawings with details of *Himantopterus fuscinervis*, an extraordinary insect from Java, described by Wesmael, in 1836, as belonging to the Lepidoptera. Dr. Hagen transferred the genus to the Neuroptera, in 1866, but Mr. McLachlan had recently examined the unique specimen in the Brussels Museum, and had decided that it was truly lepidopterous. Mr. McLachlan also exhibited leaves of a large species of *Acer* from trees growing in a garden in the neighbourhood of Brussels. Almost every leaf had been mined by the larva of a small saw-fly (*Phyllotoma aceris*), a species occurring in England. This insect only appeared in the locality mentioned last year, and yet was found by Mr. McLachlan in enormous numbers.—Prof. Westwood exhibited specimens of a minute Hymenopteron from Ceylon allied to the British *Mymar pulchellus*.—Prof. Westwood also exhibited males and females of the rare beetle *Narycius smaragdulus*, from India. This insect had remained almost unknown since the time of its description by the exhibitor in 1842.—Mr. James Wood-Mason, of the Calcutta Museum, exhibited the two sexes of *Phyllothelys Westwoodi* (*Mantidæ*), which species was remarkable on account of the presence of a large frontal horn in the female not represented in the male.—Mr. Wood-Mason also exhibited a beautifully-executed drawing of a stridulating spider (*Mygale stridulans*) in a stridulating attitude, and likewise specimens of stridulating scorpions, from India. Mr. Mason also handed to the president for identification, an homopterous insect with what appeared to be the larva of some case-bearing lepidopterous insect attached to it.—Mr. P. Wormald exhibited, on behalf of Mr. Pryer, a small collection of Chinese Lepidoptera.—Mr. G. C. Champion exhibited some rare beetles from Aviemore, Invernesshire; among them a new British Longicorn, *Pachyta sexmaculata*.—Mr. J. Jenner Weir mentioned a case of parthenogenesis in *Lasiocampa quercus* which had recently come under his notice.—The president read a letter from Herr Grevelink, of the Hague, relating to the insect which destroys the West Indian cocoa-nut trees (*Aleyrodes cocoïs*).—The Secretary exhibited a Longicorn beetle, which had been forwarded from Birkenhead by Mr. David Henderson.—Mr. J. W. Slater read a paper entitled "Vivarium Notes on some Common Coleoptera."

GÖTTINGEN

Royal Academy of Sciences, April 23.—The dates of Genesis, by M. Oppert.

April 30.—Celebration of the centenary of Gauss's birthday.

May 5.—On the mutual relations of magnetising force, temporary and permanent magnetism, by M. Fromme.—Experiments on the apparent attraction and repulsion between bodies moving in water, by M. Schiötz.—On the same, by M. Bjerknes.—Experimental investigation on the resistance of flames to the galvanic current, by M. Hopper.

July 7.—Demonstration of a tangent multiplier constructed on a new principle, by M. Riecke.—Remarks on some transformations of surfaces, by M. Enneper.—On the border-angle of the expansion of liquids on solid bodies, by M. Quincke.—On geometrical extensions of the Bezout fundamental law, by M. Schubert.—On the structure and systematic position of the genus Carludovica, by M. Drude.—Communication on the pyroelectricity of tourmaline, by M. Hoppe.

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